

The Origins of Behaviourism

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for Lynne

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Abstract

The psychological movement known as behaviourism has a unique importance as a case study in scientific methodology. Although the movement was founded in large part upon an aggressively objective approach to the problems of psychology; received the allegiance, research efforts, and personal sympathy of the majority of psychologists and logicians who came in contact with it; continued both to attract new adherents and to develop its investigative methodology throughout the period of its chief influence; and, most important, served in one way or another as the basis for a truly enormous amount of careful and sophisticated research, it nonetheless never managed to produce a significant and workable body of scientific knowledge at all comparable to that available in most other scientific disciplines. In order both to shed some light on the movement's relative scientific failure and to determine some of the implications of that failure for the further systematic development of psychology, the present study traces and analyses the characteristics of and background to behaviourism as a self-conscious movement in psychology.

The study alternates between historical and broadly logical analysis. Chapter 1 documents the current decline of behaviourism and introduces the problem of determining what it was that led to its decline. Chapter 2 considers and rejects the thesis that behaviourism was (or had) a substantive 'paradigm' and that it is currently undergoing the kind of scientific revolution described by T. S. Kuhn; instead, it is suggested that what was central to behaviourism was methodological objectivism, the conviction that methodological insights, rather than theoretical or substantive ones, are most important for ensuring scientific advance. Chapter 3 extends this last point and shows how

the methodological conception of science typical of behaviourism can be described generally as a 'positivist' one. This chapter then analyses the characteristics, potentialities, and limitations of positivist approaches to science, and concludes that such approaches are appropriate when the focus of scientific interest is on the terms and concepts of a scientific theory as such (the 'context of reconstruction'), but are not appropriate when the focus of scientific interest is on the problems and phenomena which the theory attempts to explain (the 'context of construction'). Chapter 4 examines in some detail the circumstances of the founding of behaviourism and of its acquiring a positivist orientation; these circumstances were related to the insuperable difficulties involved in doing animal behaviour research on the introspective model characteristic of functionalist comparative psychology. The rejection of consciousness and introspection in comparative psychology was an appropriate response to these difficulties; however, the generalization of this response to the rest of psychology was based on an entirely unwarranted appeal to the standards of objectivity thought characteristic of the natural science. It was this appeal, and the results of its enormous success, that determined the subsequent methodological constitution of behaviourism. Chapter 5 begins by tracing the gradual transformation of this methodological constitution from its initial, almost implicit version in early or classical behaviourism to its eventual sophisticated and explicit version in neobehaviourism. This chapter then makes a general analysis of methodological approaches to scientific inquiry, at a somewhat more technical level than was done in Chapter 3, and attempts to show that such approaches are necessarily unsuccessful in the long run, because methodological rules, or decision procedures, cannot be

made to apply satisfactorily to many of the types of problems characteristic of scientific research. This analysis is then applied specifically to some of the characteristics of behaviourist research. Chapter 6 concludes the study with the judgment that the fundamental systematic contribution of behaviourism to the ongoing development of psychology lies in its demonstration that the methodological principles on which it was largely based are untenable, but as a consolation suggests that behaviourism's unsystematic contributions, relating to the training of psychologists in ways of seeing, are greater than have been appreciated.

Chapter 1

The Decline of Behaviourism

Whether behaviourism is considered to be a psychological system, a scientific methodology, a set of psychological theories, a group of orienting assumptions, or a metaphysical commitment about the nature of man, it is clear that it is gradually losing whatever pre-dominance it once had in psychology. Since behaviourism has always been a loose coalition of interests rather than (or as well as) a specifiable body of theory and research, it is difficult to document its decline with any precision. Several indicators stand out relatively clearly nevertheless, although some of them are admittedly impressionistic and almost all are somewhat overlapping. We may list five that seem to be among the most important ones. First is the publication, in growing numbers, of increasingly sophisticated methodological and conceptual critiques of behaviourism. Second is the loss of external philosophical support for behaviourism. Third is a tendency towards attrition in the ranks of both well-known and unknown behaviourist psychologists. Fourth is the resurgence of interest in various kinds of explicitly 'mentalistic' psychology. Finally, fifth is a gradual change in the kind of articles published in the hitherto mainstream behaviouristic journals.

The refutation or rejection of systematic theories associated with behaviourism is not separately mentioned in this list. Criticism of the approaches or orientations exemplified by the theories can be subsumed under the other headings, while the theories themselves, particularly the more comprehensive and systematic ones, were for the most part abandoned long ago.

The present chapter will briefly examine each of these indicators in turn, not with any intent of making a comprehensive survey or assessment of them, but merely to give some picture of the overall pattern of behaviourism's decline as it has emerged in recent years. The chapter will conclude with a preliminary discussion of the significance of behaviourism's decline, and of the relevance of a detailed historical and critical appreciation of it, for the development of post-behaviourist psychology.

I. Indices of Behaviourism's Decline.

General Methodological and Conceptual Critiques.

First, the level of methodological and conceptual critiques of behaviourism is considerably higher than it used to be. During the early heyday of behaviourism, unsympathetic general critiques of it tended--with some notable exceptions--to be naive and overly simplistic. They often did not attempt to assess behaviourism within its own, or even a common, frame of reference, but argued instead that behaviourism was simultaneously undesirable and inconceivable. They thereby left themselves open to charges that they were a priori positions and as such not to be taken seriously. More recent critiques take behaviourism's empirical findings and methodological import much more seriously and examine them in much greater detail; they thereby focus their criticisms on precisely those aspects of behaviourism which its chief proponents have always, quite rightly, claimed to be central to it. These critiques have as a result been considerably more effective than the earlier ones.

Peters (1958), for instance, has made a closely focused examination of the internal structure and empirical findings of some modern psychological theories of motivation, especially psychoanalytic

theory and certain behaviourist (mainly Hullian) theories. He shows that a careful examination of the structure of Hullian theory reveals that the theory is inconsistent specifically as a formal hypothetico-deductive theory. This purported feature of the theory had always been taken to be one of its major virtues, because it facilitated unequivocal tests of the theory's validity. Furthermore, Peters shows that if predictions from the theory are derived rigorously, the empirical findings provide, at the very best, equivocal support for it. Concerning his insistence in his monograph, on treating psychological theories for better or worse as rigorous systems, Peters notes:

Some psychologists have praised the Monograph for discussing psychological explanations with much more knowledge of theories actually advanced by psychologists than is usual with philosophical critics of psychology. But they have suggested that I have taken such theories too seriously! They are to be seen as moves in a game, not as ex cathedra pronouncements (Peters, 1958; 1960 ed., p. viii).

The implication is that motivational theories, including Hull's vaunted hypothetico-deductive theory, are capable of hanging together only if one does not look at them too closely. However, as Peters continues:

I am well aware that psychologists are very critical of ambitious theories like those of Freud and Hull and that these have been replaced by a bewildering amount of piecemeal theorizing...But I think that these theories are still very influential. For though they are outmoded as Grand Plans, their concepts still persist in the fragments of their monolithic structures. And the new piecemeal theories, because they employ many of the old concepts, still carry with them many of the more general and more unacceptable implications of the ambitious theories in which these concepts had a natural home (ibid., p. viii).

Peters' own position is that the plurality of types of motivational constraints and directives on behaviour--criminal and civil law, tissue need satisfaction, desire for self-consistency, self-actualization, Oedipal conflicts, phobias, other fears, long-term

goals, etc.--have logically different statuses and thus cannot be reduced to one single principle. Hence, any all-embracing motivational theory with universally applied explanatory principles is logically barred from success. Nevertheless, a major proportion of human behaviour can be subsumed under a purposive, rule-following model, of a sort which has been almost totally neglected in most major motivational theories.

Psychology has not soared into its Galilean period as is often thought, through lack of bright ideas, experimental ingenuity, or methodological rigour. It has remained earthbound in mazes and Skinner boxes because the highly general theories which, it was hoped, would emerge, are logically impossible. The fundamental mistake of theorists like Lewin and Hull was to assume that what psychology requires is a Galileo. What would be much more salutary would be a more careful scrutiny of the conceptually illuminating start made by Aristotle (*ibid.*, pp. 156-157).¹

Taylor (1964), like Peters, favours replacement of the loosely mechanistic and non-cognitive explanatory schemes typically favoured by behaviourists with an Aristotelian kind of explanation. Such an explanation would emphasize the roles of purposes, intentions, and expectations--explicitly teleological factors--as determinants of behaviour, or at least of normal, non-pathological behaviour. Taylor stresses that, contrary to the frequent claims of behaviourist psychologists and positivist philosophers, the relative merits of teleological and mechanistic explanation can be assessed empirically. Teleological explanation does not, as is frequently alleged, depend upon a causative action being exercised by a future event. It merely relates present behaviour to a future goal or end state by characterizing the behaviour as a sufficient condition for the attainment of that end state. The behaviour can then be 'explained' as a function (but not specifically as a backwards causal function) of the goal to

which it leads. The choice of variables in terms of which to account for behaviour is a broadly empirical question; the variables chosen can be prior to, concomitant with, or subsequent to the behaviour to be explained without the explanation in any case involving the action of a causative factor independent of observable phenomena.

With this defence of teleological explanation Taylor neatly turns the tables on those 'hard-headed' psychologists and philosophers who maintain that teleological explanations are inadmissible in principle, because they involve backwards causation, animism, and other occult fancies. The proper response, which Taylor illustrates but does not explicitly state, is that this objection applies equally to any attribution of causal agency over and above observed phenomena. The Humean strictures against causality per se do not provide an exclusive warrant for explanations in terms of "constant conjunctions of events" that are related in one temporal direction rather than the other. Taylor analyses the structure of teleological explanation in some detail in order to justify his use of it in this way, and then focuses on the problem areas and empirical findings of behaviourist learning theories. He concludes that, if teleological explanations are granted an initial credibility equal to that automatically given to mechanistic explanations, then even in the problem areas most intensively studied by behaviourist psychologists, the empirical findings support teleological explanations at least as well as, and sometimes better than, mechanistic ones.

Teleological explanations have provided a favorite alternative to behaviourism, particularly for British philosophers, for many years. The significance of critiques such as Peters' and Taylor's is that they make detailed contact with fully developed behaviourist

theories and, particularly in the case of Taylor's, advance a well thought out alternative that applies to the very problems which behaviourist psychologists have most intensively investigated.

Apart from a teleological orientation, another profitable basis for criticism of behaviourism has been (Chomskyan) psycholinguistics and generative grammar. Chomsky's review (1959) of Skinner's Verbal Behavior (1957) is the prototype of this kind of criticism. In his review, Chomsky argues convincingly that whatever may be the utility of S-R and reinforcement models for explaining some instances of animal behaviour, application of these models to most human behaviour has received almost no experimental justification. On the contrary, there are strong empirical, logical, and epistemological grounds for concluding that they are inapplicable to most behaviour that is distinctively human. The most compelling of these grounds is that the properties of language, and the patterns of typical language use, are such that the system in which they are embedded must be partially non-deterministic (at least in any simple sense) and stimulus-independent (see Chapter 2, pp. 40-42). Fodor's well-received book, Psychological Explanation (1968), is based largely on Chomsky's formulations. Fodor concentrates on what he considers the fundamental logical assumption of behaviourist psychology, that of the necessary validity of logical reduction of mental predicates to behavioural ones. He concludes that as a logic claim this assumption is question-begging, since it provides a rationale for ignoring empirical data both selectively and a priori; and as an empirical judgment it is simply false.

From a third perspective, von Bertalanffy (1970, 1971) has argued that behaviourism, psychoanalysis, and much of cybernetic theory

are equally objectionable in that they all rest on the gratuitous assumption that the human organism is a passive reactor to external stimulation. He maintains that general systems theory, if it is not misinterpreted², provides a validated scientific concept of man that does justice to the scientifically observed data and the phenomenal experience of personal autonomy, personality integration, creativity, etc., and that does so without a retreat into vitalism or any kind of mind-body dualism.

Sophisticated modern critiques of behaviourism may be said to have had their start with publication of the cooperative volume, Modern Learning Theory (Estes et al., 1954). This volume was not meant to be destructively critical. Instead, its purpose was to provide a

...critical examination of contemporary learning theory which sought to make the best use of past experience as a basis for evaluation, and to draw some lessons for the further development of learning theory...to clarify differences among current theories and to abstract features which they hold in common (Estes et al., 1954, pp. xi-xiii).

However, the enterprise led to somewhat different results from those optimistically expected of it, particularly in the volume's longest essay, Koch's analysis of Hull's learning theory. While he initially undertook the assignment in all good will, Koch eventually came to demonstrate that a) there were systematic contradictions in Hull's theory, in that different predictions for the same behavioural situation could be derived from different combinations of theorems; b) Hull's hypothetical constructs (e.g., ' r_G ', ' S_D ') had an irreducible and unadmitted ontological import, in that they had an independent explanatory function but were fully specifiable neither as postulates nor in physiological or behavioural terms; and c) the numerical values of the parameters in the theory were insufficiently determined

and were in some cases almost arbitrary.

Koch's essay was the first full-scale, internalist, formal analysis of a major behaviourist theory. He has recently described it, somewhat immodestly, as "probably the most mercilessly sustained analysis of a psychological theory on record (Koch, 1971, p. 2)." Since then, analyses in a similarly critical spirit have become somewhat more commonplace, but Koch's original analysis has become outdated only in the mildness of its tone. Succeeding analyses of behaviourism, including Koch's own, have become far more trenchant. Indeed, Koch has established what amounts almost to a second career in his criticisms and eventual denunciations of behaviourism (e.g., Koch, 1954, 1959, 1962a, 1964, 1969, 1971). He describes his own relationship to the movement as one of apostasy (Koch, 1969). The pattern of his analyses of behaviourism is much the same as that of the others described here: logically, behaviourism doesn't hold together, and empirically, it doesn't work. Koch has concentrated much of his attention specifically on a criticism of operationism and of the intervening variable paradigm. In regard to operationism and related meaning criteria, he makes the point which will be further developed below, that the epistemological basis for these positions has been refined out of existence by the same philosophers who developed them, due to the inherent difficulties and implicit contradictions within the positions as originally formulated. Concerning the intervening variable paradigm, he demonstrates that contrary to the early belief that its rigorous application would constitute a relatively easy means of ensuring objectivity in theories, intervening variables are in fact almost impossible to establish empirically in any way that will guarantee their trans-situational applicability. Furthermore, even

in the rare cases in which they can be unambiguously defined, the definition imposes such constraints on them that while they possess some trans-situationality, they have little real generality, and consequently far less utility than was once supposed. In terms of general orientations, Koch's analyses differ from the others mentioned here mainly in that he is writing very explicitly from within the tradition which he is criticizing, and in that he is not trying to expound a systematic alternative³.

Loss of External Philosophical Support.

In addition to more effective activity on the part of its philosophical enemies, behaviourism has also suffered the loss of its philosophical friends. The quite extensive logical and philosophical support which it once enjoyed has by now almost entirely evaporated. Logical positivists such as Bergmann, Carnap, and Feigl all made important contributions to the logical foundations of behaviourism (e.g., Bergmann, 1951; Bergmann & Spence, 1941; Carnap, 1936; Feigl, 1934, 1945). The physicist Bridgman (1927) made what was perhaps an even greater contribution in formulating the principles of operational analysis or definition, principles which became very influential in behaviourist experimentation and theory construction--notoriously, much more influential than they were in physics.

However, as part of the general decline of logical positivism and related positions in recent years, all of these philosophers have more recently taken positions which are incompatible with, or at best irrelevant to, behaviourism. Bergmann's latest book (Bergmann, 1967) formulates a realistic ontology of acts and processes that has close affinities with the intensionalism of Brentano and Meinong⁴. Carnap's later attempts to construct criteria for meaning and theory construc-

tion (Carnap, 1956) have been described by Koch (1964, p. 22) as "so liberalized...as to make them compatible with certain classes of metaphysical statements." Feigl (1967, p. 60) cites this same paper by Carnap, as well as several others including some of his own recent ones, as jointly establishing that "the thesis of the translatability of statements about mental states (in phenomenal language) into statements about peripheral behavior...must also be repudiated."⁵ Feigl himself has, in addition, come to insist on the significance of the problem of other minds, the importance of the mind-body problem, the necessity for consideration of purely private experience, and the validity and value of careful introspection.

Introspection, though admittedly often unreliable, does enable us to describe elements, aspects, and configurations in the phenomenal fields of direct experience. When the doctor asks me whether I have a pain in my chest, whether my mood is gloomy, or whether I can read the fine print, he can afford to be a behaviourist and test for these experiences in a perfectly objective manner. But I have (or do not have) the pain, the depressed mood, or the visual sensations; and I can report them on the basis of direct experience and introspection (Feigl, 1967, p. 5).

Bridgman (1959) has continued to maintain the value of operational procedures in science, but now promotes them primarily as a loose heuristic, so that one can know what one is doing. He has rejected altogether his earlier position that their function was largely to make science a purely public activity, and now insists that no set of procedures can make science public, that scientific activity is necessarily private and even subjective.

The evolution of and changes in British analytic philosophy are of less importance in this context, because behaviourist psychology never relied on analytic philosophy to anything like the extent to which it relied on logical positivism and operationism. It may be

of interest in passing, however, at least as a reflection of the zeitgeist, to contrast Ryle's The Concept of Mind (1949), which more or less proved the validity of behaviourism on loosely analytic grounds, with Winch's The Idea of a Social Science (1958), which more or less disproved it on much the same grounds. The 'behaviourisms' discussed in these two books are not quite the same, however, and neither makes very much contact with the behaviourism of American experimental psychology.

British analytic philosophy aside, the significance of this loss of philosophical backing for behaviourism cannot be overemphasized. While it is an exaggeration to see behaviourism as nothing more than a logic claim, logical analyses were of central importance to it. In the first place, the logical positivist analyses established, or were held to establish, the practicability and necessity of basing scientific formulations strictly on publically observable events. This demonstration was of great utility in providing an independent justification for a psychology based entirely on observations of behaviour rather than of, say, states of consciousness. Second, the analyses specified the ways in which complex theories, inevitably containing terms which did not have an immediate observational referent, could be devised without sacrificing the commitment to observable behaviour. Hence they enabled behaviourism to go beyond mere descriptive formulations (which were the main product of its first fifteen years), and construct, or attempt to construct, genuinely explanatory theories. Third, and less formally, the incorporation of rigorously scientific logical analyses--which were held to derive from and relate to the practices of the physical sciences--served to guarantee behaviourism's own rigorously scientific character. Now, however, all three of these important supports for and

justifications of behaviourism have been withdrawn. Furthermore, they have been withdrawn precisely because the philosophers who formulated them, and later philosophers who extended them, have almost without exception concluded that they are invalid as they stand, and that it is practically impossible to make them both consistent and workable (these matters will be discussed at length in Chapter 5).

Attrition in the Ranks of Behaviourists.

The third indicator is a slight but clearly discernible trend for behaviourists to stop being behaviourists and to start being something else. Since this process is visible only when the behaviourists concerned are very well known, and since well known and productive scientists of any persuasion tend to have a strong commitment to the systematic orientations which were the basis for their achievements, the few behaviourists who have explicitly changed their orientation to psychology have a significance beyond their small number. There are strong factors which might be expected to prevent any behaviourists, or other scientists of major stature, from changing their allegiance.

The most vociferous of those who have recanted is Koch, whose career of self-styled apostasy was mentioned above. Palermo (1970, 1971) has also made a public conversion. He considers that behaviourism is a scientific system that has had a good and productive run, but has now run its course and is due to be naturally replaced by a successor. He sees his own position as part of this general shift, moving along with the rest of experimental psychology from behaviourism to a broadly conceived Chomskyan mentalism⁶.

The first prominent behaviourist to make a radical change in orientation was perhaps Mowrer (if we discount figures such as Lashley, who moved from vigorous S-R formulations to equally vigorous S-S form-

ulations). In Mowrer's case, however, the change occurred so gradually that it is impossible to specify just when or how it happened. It clearly did happen, nonetheless. Mowrer achieved prominence in psychology originally with his extension and later adaptation of Hullian learning theory. His 'two factor' theory proposed, basically, that emotional (autonomic) responses are learned through a process of classical conditioning, and motor responses through a process of instrumental conditioning (Mowrer, 1947). His more recent work has emphasized personal accountability and responsibility in neurosis, and the validity of the concept of 'sin' in psychotherapeutic self-evaluation (Mowrer, 1960, 1966). He now sets himself explicitly against what he sees as the trend of modern behaviourism in his uncompromising emphasis on individual autonomy and the potentials for self-direction of behaviour (Mowrer, 1972).

Meehl, while never a whole-hearted behaviourist, was very sympathetic to the movement. He was the author of an influential paper justifying the logical status of the law of effect (Meehl, 1950), and the co-author of an even more influential paper on the logic of psychological theories--the theories in question being exclusively behaviourist ones (MacCorquodale & Meehl, 1948). He has since concerned himself, while staying active as a methodologist and clinician, with formulating an empirically meaningful concept of the evolutionary emergence of mental properties (Meehl & Sellars, 1956), and with attempting to demonstrate the possibility and resulting efficacy of free will and faith on the basis of sophisticated philosophical analysis (Meehl et al., 1958).

Three other major figures may be mentioned who have not

abandoned their behaviouristic stand, but who have tacitly minimized its importance in their thinking to a greater or lesser extent. Cofer has recently judged that drive-reduction varieties of motivation theory--with which he was long associated--are about to be replaced by explicitly cognitive theories, and he implies at least that he approves of the change (Cofer, 1972; see p. 18 below). Finally, Krech and Miller --who were at one time seriously engaged in extension and elaboration of the theories of Tolman and Hull, respectively--have made no compromise in their aggressively hard-headed objectivist orientation, but seem to have abandoned their hopes for profitably applying their experimental rigour to the construction of behavioural systems as such, and are now concerned primarily with psychobiology.

The Resurgence of Mentalism.

'Mentalism' is a word used disparagingly by behaviourist psychologists to refer to any psychological theory or orientation that places central importance on conscious experience or mental events, especially when these are conceived of as determinants of, and prior to, observable behaviour. Tolman, while referring particularly (although not explicitly) to the introspective structural psychologies of Wundt and Titchener, provides a good example of such usage.

The mentalist is one who assumes that 'minds' are essentially streams of 'inner happenings.' Human beings, he says, 'look within' and observe such 'inner happenings.' And although sub-human organisms cannot thus 'look within,' or at any rate cannot report the results of any such lookings within, the mentalist supposes that they also have 'inner happenings.' The task of the animal psychologist is conceived by the mentalist as that of inferring such 'inner happenings' from outer behavior; animal psychology is reduced by him to a series of arguments from analogy (Tolman, 1932, p. 3).

As such, 'mentalism' has close functional connections (for behaviourists) with other favourite pejoratives such as 'animism' and 'mysticism'⁷.

The minimal behaviourism-oriented characterization of 'mentalism' that opened this paragraph is apposite however, because it applies to a large and increasing proportion of contemporary psychology, the different segments of which often have little in common apart from their non- or anti-behaviourist 'mentalism' as so conceived⁸.

Much of this resurgence of mentalism is associated with a loose cluster of movements or schools comprising existential psychology in all of its ramifications, humanistic psychology, and phenomenology. Existential psychology ranges over the anti-cultural indictments of Laing (1962; Laing & Esterton, 1965) and, less directly, Szasz (1961, 1970), the descriptive psychopathology (daseinanalyse) of Binswanger (1958), the normative existential Marxist idealism of Fromm (1964), the later personalistic developments of client-centred therapy (Rogers, 1961, 1964), the organismic theories of Maslow (1962) and May (1953), and many others (for a survey and guide to the literature, see May, Angel, & Ellenberger, 1958). Most formulations of existential psychology are based on clinical insights, but are intended, like Freudian theory, to have valid application far beyond the bounds of clinical practice. Humanistic psychology is similarly a mixed bag, and is if anything even more diverse in its manifestations. The best introduction to the range of humanistic psychology is the anthology edited by Bugental (1967), with representative essays by Bugental, von Bertalanffy, Koestler, J. R. Royce, Lifton, and a number of others, including some of those identified above as existential psychologists. Psychological phenomenology is less diffuse than the other two, although it overlaps considerably with existentialism. The classic modern works in the field are those of Merleau-Ponty (1942, 1945); recent representative

examples from it include Giorgi's introductory text reinterpreting psychology on the basis of phenomenological principles (Giorgi, 1970), Wilshire's phenomenological interpretation of James' Principles of Psychology (Wilshire, 1968), and the ongoing series of Duquesne Studies in Phenomenological Psychology (Giorgi, Fischer, & von Eckartsburgh, 1971). Somewhere in the middle of these three movements, themselves not always clearly separable, are activities such as transactional psychotherapy (Berne, 1963), Gestalt therapy (Perls, Hefferline, & Goodman, 1965), and, more broadly, encounter groups, sensitivity training, etc. The purpose of this parade of sources is simply to suggest the range and diversity of modern existential types of psychology. Existential psychology and its allies are certainly not new, but the extent of interest in them is greater than ever before, and is continuing to increase. What unites all of these disparate positions, and justifies mentioning them together, is their shared conviction that close study of the contents and structures of conscious experience, including (or especially) one's own conscious experience in real-life situations, is the single most essential requirement for understanding both the human mind and human behaviour. Other varieties of mentalistic psychology rely much less on immediate experience, and often tend to treat mental entities as loosely inferred constructs.

Non-existentialist varieties of mentalistic psychology are in some ways more relevant here, as they fit more neatly into the kind of 'academic psychology' classification dominated for many years by behaviourism. As such, they are more direct competitors of or alternatives to behaviourism. The most important development in this field may indeed be Chomskyan psycholinguistics, which Palermo (1970, p. 416)

tentatively identifies as central to the more "mentalistic and rationalistic orientation" that is about to replace behaviourism. However, while Chomsky's inspirational value for those who are searching for an alternative to behaviourism may be indeed great, the direct relevance of Chomsky's generative grammar to the construction of a general psychology has yet to be shown, and may reasonably be doubted⁹. Other varieties of mentalistic psychology are often loosely called 'cognitive', another usage stemming from Tolman, although there is little continuity between Tolman's views and contemporary cognitive theories. Personal construct theory (Kelly, 1955; Bannister & Fransella, 1972) is a good and currently popular example. Another, although as yet more limited, is intentionalist motivational theory of the sort elaborated by Irwin (1971). Irwin makes a formal analysis of intentional (consciously chosen) behaviour in terms of preferences and differential outcome expectations. He maintains that drive reductionist and similar theories of learning and motivation are not so much false as grossly irrelevant to all but a tiny fraction of human behaviour. A survey by Ryan (1970) indicates that intentionalist approaches comparable to Irwin's are becoming increasingly popular.

In addition, there has been a mentalistic or cognitive undercurrent active on the fringes of experimental psychology for some years. Perhaps the best example of such a fringe activity is the series of studies by Spielberger and his associates purporting to show the necessity for awareness of reinforcement contingencies on the part of subjects undergoing verbal conditioning (e.g., Spielberger, 1962; Spielberger & DeNike, 1966). Such activities, and instances could be multiplied (e.g. studies of perceptual defence), remained firmly anchored to the typical behaviourist emphases on reinforcement, parametric

measurements, operational definitions, etc. Thus, while in their very respectability they undoubtedly eased the way for mentalistic formulations to become more acceptable in the mainstream of American psychology, they were never productive of wide-ranging, uncompromisingly mentalistic theories, as in their different ways the researches of Kelly, Irwin, Maslow, Chomsky, etc., have been.

Cognitive dissonance theory (Festinger, Riecken, & Schacter, 1956; Festinger, 1957) and some aspects of achievement motivation theory (McClelland, Atkinson, Clark, & Lowell, 1953) could also be described as more or less mentalistic fringe activities. Their inclusion is more tenuous, however, inasmuch as they are more a part of social psychology, which was never dominated by behaviourism to quite the extent typical of learning and motivation theory.

It is very difficult to assess other than impressionistically the significance and extent of the new mentalism, and impossible to predict whether some of it, and if so which part, will develop into a viable general psychology. That it is indicative of widespread and lasting changes, however, seems to be agreed by many psychologists¹⁰. Cofer, in reviewing Irwin's book on intentional behaviour cited above, observes that drive reductionist and similar theories of motivation

...make the organism like a ship tossed at sea by winds and waves while various explosions occur in its hold. A cognitive psychology, however, places agency in the hands of the helmsman or engineer, who perceives alternatives and chooses that one whose outcome he prefers, the one that he anticipates will enable the vessel to ride out the storm and the internal stress...It looks as if an era has ended, and a new one, involving notions like choice, intention, and volition, is beginning to emerge (Cofer, 1972, p. 474)

But the new era, if there is indeed going to be one, has not yet quite arrived. A recent book by Lohr (1971) maintains a sophisticated form of neobehaviourism, equally Hullian and Hebbian; Lohr attempts to

account for all cognition, affect, and behaviour, on the basis of drive reduction and the action of cell-assemblies in the brain. In commenting on Lohr's book, only a few pages after Cofer's resignedly optimistic judgment just quoted, Rychlak concludes:

...it is ironic to recall the day when an eminent scientist like Robert Oppenheimer stood before us at an APA convention and said 'in so many words' that he wished we psychologists would stop basing our fledgling science on the rapidly fading Lockean-Newtonian models of yesteryear. He hoped we would soon begin thinking of human behavior in purposive terms, even though we could not weigh, measure, or 'see' such sublime aspects of the human condition. Many other equally sophisticated empiricists in our sister sciences are waiting for psychology to make this effort. Best of luck to Lohr and others in psychology who wish to go on arbitrarily taking intention and self-direction out of man's mind. But where are our innovators? When will our Kuhnian revolution begin? (Rychlak, 1972, p. 491)

Change in the Contents of Journals.

The significance of the mainstream American psychological journals such as the Psychological Review, the Psychological Bulletin, and the Journal of Experimental Psychology, is that they are jointly the main vehicle of that mainstream, and as such provide reliable information about its direction. It is much easier to have wayward theoretical and experimental papers published in relatively minor journals such as Psychological Reports or the Psychological Record--and this fact is hardly to their discredit--than in the major journals mentioned. These major journals are slow to change, sometimes frustratingly so, and as a result changes in the types of articles which they accept for publication provide a conservative but reliable indication of basic changes in American psychology itself. Thus, the changes in journal contents described in this section do not constitute a separate index of the decline of behaviourism, or a separate set of criticisms of behaviourism. Rather, they are a microcosm of the trends already discussed, insofar as these trends have already changed the

course of American experimental psychology.

The main change in the contents of these journals has been a result of what both Koch (1964) and Smith (1969) have called the 'return of the repressed', the renewed consideration of complex human functioning, conscious processes, and higher activities such as love and curiosity, all of them put off until an indefinite time in the future early in behaviourism's career. Some of the background to these developments should be mentioned. The 'repression' of these higher processes in American psychology was carried out as a result of the early behaviourist rejection of their status as autonomously real functions or events, and of the substitution for study of them of a faith that the behaviour traditionally taken as indicative of them could eventually be accounted for in a scientific way, once behaviourist psychology had fully investigated the simpler functions from which they were held wholly to derive. The 'return' of these repressed problems was occasioned initially by the gradual realization that the very great progress which had been made in laboratory studies of restricted animal and human behaviour, and in the construction of theories based on such studies, had been accompanied by no comparable progress in relating the laboratory findings to accounts of the higher and specifically human functions as traditionally conceived. Hence, the research programme of much of what Koch (1964) calls 'neo-neobehaviorism' was devoted to investigating these problems as far as was possible with the theoretical and explanatory tools already at hand. This programme thus constituted an application of current restricted methods and limited theories to complex human functioning rather than, as had originally been predicted and hoped, an experimentally warranted extension of established principles to the explana-

tion of higher behaviour.

The significance of this shift in emphasis, and the tenuousness of it, is highlighted by the fact that the higher processes and complex functions investigated or explained in this way comprise almost the entirety of human and animal behaviour in ordinary life situations. That is, the experimental studies on which the theoretical extrapolations were based were themselves directly applicable to the explanation of practically no real-life behaviour. Peters underscores this point in discussing Hull's motivational theory.

Hull (1943) boldly proclaimed his programme of starting from 'colourless movements and mere receptor impulses as such' and eventually explaining everything in terms of such concepts--

'familial behaviour, individual adaptive efficiency (intelligence), the formal educative processes, psychogenic disorders, social control and delinquency, character and personality, culture and acculturation, magic and religious practices, custom law and jurisprudence, politics and government and many other specialized fields of behaviour.'

In fact Hull developed some simple postulates which gave dubious answers to limited questions about particular species of rats. He never asked, let alone tried to answer, any concrete questions about human behaviour. He was in love with the idea of a science of behaviour; he was not acutely worried about concrete questions of explaining human behaviour (Peters, 1958, pp. 2-3).

However, the 'neo-neobehaviourists' took it as their role to answer many of these same questions that Hull, perhaps wisely, left to the indefinite future; and they did so with very little more in the way of established behavioural principles than those which were known to Hull¹¹. As a result, the neo-neobehaviourist programme was marked by much looser use of typical learning-theory concepts than was characteristic of their use in the animal laboratory, to the extent that the terms used often seemed to have little more than analogical or even metaphorical significance. Dollard's and Miller's Personality and

Psychotherapy (1950), Skinner's Science and Human Behavior (1953), and Staats' and Staats' Complex Human Behavior (1963) may be cited as among the more fully developed attempts at such universalization of very limited behaviourist theories. A more recent attempt in the same vein, one that attempts to make a behaviourist translation of avowedly mentalistic concepts, is Smith's Behavior and Conscious Experience (1969; see Mackenzie, 1971, for critical comments).

Thus, consideration of consciousness and other 'higher' processes was originally, and at times is still, cast in a loosely behaviourist mold. The very looseness, however, facilitated the development of a diversity of viewpoints, some of which came to pay no more than lip service to the behaviouristic presuppositions which were their original basis, while otherwise proceeding quite independently. The studies of Harlow (1953) and Berlyne (1960) on curiosity, and Harlow's later research on love (1958, 1962), are examples of such divergent, if still barely, behaviourist-style formulations. Others include the classic studies of P. T. Young (1955, 1959) on the strong taste preferences in rats, sufficient to lead to the abrupt reorganization of major patterns of behaviour. Other research programmes became formally opposed to the contents of theories identified as behaviourist, although they continued to conduct their research according to the methodological precepts, and in terms of the theoretical variables, originally devised out of and for behaviourist research. Spielberger's research on awareness in verbal conditioning, mentioned above, is an example of such work.

Finally however, this developing pluralism of research orientations has come to facilitate consideration of higher processes in quite an autonomous manner, with even in the major journals no

more than a fleeting backwards glance at behaviourism. Thus, Huttenlocher (1968) has proposed a model intended to account for syllogistic reasoning, according to which subjects solve syllogistic problems relating to class-inclusion by a three stage mental operation. They first translate the syllogistic relationships into perceptual ones (e.g., relations of larger-smaller); then form mental images corresponding to the terms of the syllogism (e.g., images of beakers or Venn diagrams); and finally order the mental images according to their perceptual relationships. None of the mental operations between receipt and solution of the problem are expressed behaviourally; support for the theory is derived from predictions (which by and large are confirmed) concerning the relative difficulty of different problems¹². As another example, Shallice (1972) has proposed a model of consciousness and conscious processes that attempts to link phenomenological insights with computer-based information processing models of cognition. Sperry (1969, 1970) has developed a model of consciousness as an emergent and functionally significant gestalt property of neural circuits. Perhaps most remarkable, because it represents a confluence of several independent and well-developed research programmes, is a symposium on the extent, dimensions, and functional significance of mental imagery in children's learning, recently published in the Psychological Bulletin. D.S. Palermo, one of the symposium participants, emphasized the significance of the symposium as an indication of changing priorities in experimental psychology.

Some fifteen years ago, when I was a year from completing my graduate work...proposing a symposium on imagery at a psychological convention might have been considered a joke. Most hard-nosed experimental psychologists probably would not even have set aside their copies of Modern learning theory...long enough to notice such a symposium (Palermo, 1970, p. 415).

II. The Significance of Behaviourism's Decline.

The Present Status of Behaviourism.

The trends discussed above jointly give a picture of a science that has seen its systematic theories long since rejected or abandoned, its methodological base seriously eroded, many of its ablest practitioners lost to other fields or orientations, and its hegemony over the practice of experimental psychology effectively curtailed. So far, no attempt has been made here to make any original criticisms of behaviourist psychology, because the concern has been specifically to review the scope and influence of previous criticisms and other trends that reflect on behaviourism's status. Behaviourism can at present be judged fairly to have failed, for whatever reasons, in its systematic attempt to develop a comprehensive, consistent, unified, and workable psychology. Behaviourism is by no means dead of course, but it is now what it has never before been since it achieved its early prominence--merely one way of doing psychology among many others, and no longer even the obviously most progressive, dynamic, or important way. For an approach to psychology that was long proclaimed to be the only scientific way of doing psychology, that was at least in experimental psychology typically accepted as such, and that was often considered in fact to be synonymous with scientific psychology--for such an approach all of this marks a serious comedown. The comedown is sufficiently serious that it can be taken to mark the end of behaviourism as a systematically accepted rationale for experimental psychology, the end, if one likes, of behaviourism as experimental psychology's 'paradigm' (although the term can only be used loosely; see Chapter 2). The fact that there are still many behaviourist psychologists who are still very active does not affect this conclusion. Behaviourism

as a way to do psychology is, if in decline, still viable; behaviourism as the way to do psychology is finished.

The fact that, in this sense at least, behaviourism is finished, makes it possible for the first time to regard behaviourism as a whole. That is, behaviourism can now be seen as one chapter in the ongoing history of psychology, even if a chapter which is not quite completed, rather than as the plan and goal for the entire story. The main threads of that chapter, the outlines and climax of its plot, can now be disentangled from their primary sources in the recent history of psychology. Only the denouement of the chapter is not yet visible, thereby placing one limitation on any attempt to characterize behaviourism as a whole. Any such attempt must proceed on the assumption--which seems a safe one but is inevitably subject to some risk--that the denouement will not carry with it any great new achievements and consequent rebirths of behaviourism that will require reinterpretation of the systematic status of the whole movement.

What We Can Learn from Behaviourism's Decline.

It might seem to smack of indecent haste (to change the metaphor) to attempt a dissection of behaviourism before the patient has properly expired. But it is important that the attempt be made, and preferably that it be made several times independently, before psychology finishes moving on to whatever comes after behaviourism. It is important because it is primarily through such attempts that the systematic failure of behaviourism has the potential to provide specific lessons for post-behaviourist psychology.

It is possible to make a number of general characterizations of behaviourism and of the reasons for its decline, and at least some of these will lead to differential predictions for the future course of

psychology. Furthermore, and more important for actually determining that future, they will lead to different choices of action on the part of psychologists who accept them. As more and more psychologists become disillusioned with whatever they see as the behaviourist orthodoxy, their conception of what behaviourism is--that is, of what they are reacting against--will be of considerable significance in influencing what they will do next. This consideration applies not only or even primarily to the relatively small number of disenchanted eminent behaviourists who leave the fold. It applies even more to the large number of young and presently unknown psychologists who have been exposed to a behaviourist orientation in their graduate training and early professional careers, who are dissatisfied with behaviourism, and who are looking for an alternative. Some of these psychologists will presumably become eminent themselves in another ten to twenty years. The choice of alternatives which they make, which will be in part founded on what they see their choices as alternatives to, will be a major factor in determining the direction which psychology takes in the future.

Already, it is possible to see some of the different courses of action to which different conceptions of behaviourism have led. These different alternatives are at present visible only in the actions of the eminent former behaviourists discussed previously, and hence may not be typical of those which will be adopted in the future. Nevertheless, they clearly show the extent of differences which may occur.

Koch (1969, 1971) believes that behaviourism is a fully developed scientific system, and that its lack of systematic success should be taken as strong evidence against the possibility of developing psychology as a viable systematic science of any persuasion. He

recommends the removal of almost all constraints on theorizing except for that of simple rationality (although it is not always clear how much or how little that minimal constraint would involve in Koch's analysis), and the abandonment of the fiction that psychology is or should be a single discipline. His own role is in part that of a gadfly, bringing his philosophical and historical sophistication to bear in the criticism of attempts to make psychology more than it can reasonably hope to be.

Palermo (1970, 1971) sees behaviourism, and its decline, as having somewhat more specific and limited significance. He maintains that behaviourism is a limited set of theories and assumptions centred on S-R and reinforcement models, and that most of these have been rendered untenable. He forecasts the emergence of a more "mentalistic and rationalistic orientation (1970, p. 416)" which will replace the fading behaviourist one. However, he does not feel that this 'paradigm shift' limits the prospects for scientific psychology. On the contrary, it is fully consistent with his analysis to interpret his recent work on rule-governed learning (e.g., Palermo & Parrish, 1971) as the beginning of his own contribution towards systematizing objective research in an emerging post-behaviourist tradition.

Mowrer's conception of behaviourism is more specific still. He apparently feels that what is central to behaviourism is a denial of the self-directedness of behaviour, a denial that leads to an overly rigid determinism and a resulting unwarranted abrogation of personal responsibility (e.g., Mowrer, 1972). His response, as described above, is to wage a rather lonely campaign directed at re-establishing concepts indicative of personal responsibility (such as 'sin' and 'integrity') to a more central and respectable role in psychological

description and theory.

Each of these three theorists agrees that there is a lesson to be learned from the decline of behaviourism, and they are undoubtedly right. Their work is directed to redressing what they each see as the chief inadequacies in behaviourist psychology, and such pluralism is certainly justified. Different modes and foci of inquiry are appropriate in attempts to correct the various imbalances to which behaviourism led, and it is no part of a critique of behaviourism to suggest that everybody should do the same thing as an alternative to behaviourism. Still, it would be most desirable if at least the negative lesson to be learned from the failure of behaviourism were one on which most psychologists could reach some agreement. That is, it would be desirable if they could agree on whether there was something about behaviourism in general which eventuated in its different, specific inadequacies in different problem areas within psychology; and if so, then what that general something was. This negative lesson is not simply a subjective or personalistic one, and while it may certainly be compatible with a pluralistic post-behaviourist practice in psychology--it would be of dubious utility if it were not--it can not be expected to be derivable from such practice. Rather, the lesson, if indeed there is one, will have to be derived from detailed interpretive analyses of behaviourism's central systematic characteristics.

This point can be elaborated somewhat. It is at least possible, and may be quite likely, that the failure of behaviourism to issue in a comprehensive and workable psychology can be accounted for in part on the basis of some few of its central characteristics. That is, there may have been certain determinable features of behaviourism--what Popper (1963) would in a constructive spirit describe as mistakes--

that prevented it from achieving success consonant with its expectations. If so, then it is worthwhile to try to discover what these mistakes were, in order that they might be avoided in the future without implying a wholesale rejection of everything associated with behaviourism. The goal of identifying and hopefully avoiding such mistakes is sufficiently worthwhile that it justifies making attempts, even if they are bound to be premature, to understand the movement's systematic character and limitations.

To recapitulate, different psychologists will inevitably draw different morals, learn different lessons, from the decline of behaviourism, and this is surely appropriate and desirable. But there may be, in addition, a potential lesson to be learned from the decline of behaviourism which can be of general use to the growing number of disenchanted behaviourist psychologists. If so, and if that lesson is to contain more than (while perhaps including also) a reaction against whatever about behaviourism has particularly displeased them, and a legitimization of their alternate personal preferences, then it will have to be based on frankly interpretive analyses of what it was about behaviourism which prevented it from being fully successful.

These considerations have a particular application to, and a particular cogency on account of, behaviourism's expressly and aggressively scientific character. Whatever else behaviourism was, it was the most sustained attempt ever made to develop an explicitly and self-consciously 'scientific' psychology, along the lines of 'science' as detailed in the most precise, comprehensive, and sophisticated formulations of the logic and methodology of science ever available. Nevertheless, despite or--perhaps--even in part because of its forcefully scientific orientation, it failed in its attempt to develop a consistent,

unified, and workable psychology. Since we are still concerned to develop a psychology that is in some meaningful sense scientific, it is important to us to understand what it is about behaviourism that led to its failure, and particularly, what relationship that failure had to its scientific orientation.

There are several possible ways in which behaviourism's 'scientificness' might have been implicated in its failure. Did it fail because psychology cannot, by its very nature, be a science? Or was the conception of science implemented by behaviourism an erroneous one, not really characteristic of science at all? Or was the conception of science implemented by behaviourism appropriate only to some sciences, such as physics, and inappropriate to others, such as psychology? That is, are there perhaps fundamental differences between what constitutes a science of the physical world and what constitutes a science of mind or behaviour, with behaviourism ignoring these differences to its peril? Or was there something left out so that despite all the methodological trappings of science, behaviourism was not really scientific at all? Or did it not really fail at all but, as Palermo (1971) suggests, achieve all that could reasonably be expected of it before it was replaced by a successor? These are all more or less open questions and, especially in psychology's present transitional situation, worthy of consideration. They may not all admit of a final answer, but it is clear that if one or another of the possibilities they point to should turn out to be the case, it would have significant implications for what we can expect psychology to be like, and what we can expect it to amount to, in the future. In this way, the systematic status of psychology as a science is itself brought under scrutiny through a study of behaviourism's scientific career.

The present essay does not attempt to answer all of these questions, or to give a final answer to any of them. What it will do is to examine the scientific character of behaviourism, and on the basis of that examination develop an analysis of the foundations of behaviourism. The analysis of the foundations will, in turn, hopefully cast some light on the movement's rise to prominence and its later disappointing decline.

Chapter 2

Two Views of Behaviourism

I. Behaviourism and Kuhnian Paradigms.

In reviewing a book that described cognition as S-R circuits in the brain, Rychlak (quoted above, p. 19) concluded by offering best wishes to the remaining drive reductionists and any others who continue trying to implement a behaviourist conception of psychology. "But where" he lamented "are our innovators? When will our Kuhnian revolution begin?"

According to Palermo (1970, 1971), that revolution has already begun. Palermo analyses the recent history of psychology within the framework of Kuhn's elegant treatise, The Structure of Scientific Revolutions (Kuhn, 1962). Palermo does not accept Kuhn's claim that since psychology and the rest of the social sciences are in a 'pre-paradigm' stage of development, his (Kuhn's) analysis does not apply to them¹. Instead, Palermo claims that psychology has already had two paradigms, the Wundtian (or structuralist) and the behaviourist, with a scientific revolution between them. Furthermore, he holds that the behaviourist paradigm has been in a steadily mounting crisis state for about twenty-five years, that this crisis state has engendered the beginnings of another scientific revolution in psychology, and that a new paradigm based on Chomskyan psycholinguistics and described as "mentalistic and rationalistic" seems the best bet to replace behaviourism and become psychology's third paradigm. Palermo's analysis is an elegant one, and while some tentative attempts have previously been made to apply Kuhn's insights to psychology², Palermo's is the first full-scale analysis of behaviourism based on Kuhn's thesis. In fact, Palermo's

is one of the first detailed attempts of any sort to account for behaviourism with the interpretive historiographic tools developed by professional historians of science. For these reasons, his account deserves respectful consideration. In addition, an examination of Palermo's analysis and of the reactions to it will provide an excellent introduction to some of the critical and theoretical themes central to this essay.

The Elements of Kuhn's Analysis.

The outlines of Kuhn's account of the development of science are familiar and need not be recounted in detail, but the relationships between his central concepts--'paradigms', 'normal science', 'anomalies', 'crises', and 'revolutions'--should be briefly specified. Furthermore, the concept of a 'paradigm', at least, needs to be briefly clarified.

'Paradigm' is the most central and basic concept in Kuhn's account. It is also the vaguest. The initial characterization of paradigms given by Kuhn is that of "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners (Kuhn, 1962, p. x)." Kuhn extends and elaborates this characterization in discussing the embodiment of paradigms in textbooks and classic scientific treatises. Such works

...served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. They were able to do so because they shared two essential characteristics. Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. Achievements that share these two characteristics I shall henceforth refer to as 'paradigms,' a term that relates closely to 'normal science.' By choosing it, I mean to suggest that some accepted examples of actual scientific practice --examples which include law, theory, application, and

instrumentation together--provide models from which spring particular coherent traditions of scientific research (ibid., p. 10).

Kuhn further stresses the character of paradigms as specific achievements in emphasizing that "...the concrete scientific achievement, as a locus of professional commitment, [is] prior to the various concepts, laws, theories, and points of view that may be abstracted from it (ibid., p. 11)."

The conception of a paradigm as an exemplary, concrete scientific achievement may thus be taken as basic, but Kuhn goes on to use the term in a range of varying, though related, ways at different points in his argument. The varying usage serves to focus attention not only on the scientific achievement itself but also on the general effects upon and implications for the discipline which it justifies at various removes from itself. Such effects are 'abstracted' from the paradigm achievement by processes of loose implication or of direct modelling. They include the codification of the achievement in textbooks, the principles which the achievement adduces, the methods for the study of nature which the achievement illustrates, and ultimately the conceptual framework within which the problem field is subsequently seen and interpreted as a result of repeated focusing on the achievement's role as an exemplar for the further conduct of science. All of these derived effects are at different points included by Kuhn in the concept of a 'paradigm'³.

The rest of Kuhn's central concepts are less problematic, even if no less contentious. The paradigm, in the extended sense, or the paradigm and its implications, determine the framework for the practice of normal science. Normal or everyday scientific activity is a process of extending the insights provided by the paradigm to cover the

rest of the field of inquiry. It consists largely in "an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies (ibid., p. 24)." Restricting attention to those problems and foci of inquiry suggested by the paradigm leads to two central characteristics of normal science. First is its specificity and depth.

By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable...during the period when the paradigm is successful, the profession will have solved problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm (ibid., pp. 24-25).

Second is its character of puzzle-solving rather than of problem-solving. Puzzles, unlike problems, have an assured solution, and a more or less known set of procedures for obtaining the solution. The theoretical and experimental investigations undertaken in normal science are puzzles because their solution, and the appropriateness of the established procedures for obtaining their solution, are assured by their relation to the paradigm; it is just such investigations, serving to consolidate and extend the paradigm's insights, that are chosen for study in a period of normal science. Since it is implicitly assumed that correct answers are available, it is in effect taken to be the scientist's ingenuity that leads to successful normal-scientific experimentation, and his lack of ingenuity that leads to failure. That is, failure to achieve results consonant with the paradigm-derived expectations tends to reflect on the scientist rather than on the paradigm. As contributions to the body of knowledge, failures are not taken as seriously as are successes. In this way, the continuity of scientific research can be maintained in the face of evidence that is prima facie anomalous for the paradigm-derived principles of the discipline.

But normal science is not simply an exercise in self-congratulation and self-deception. The paradigm serves to devalue individual failures only so long as these are relatively isolated. Eventually however, and precisely because of the depth and intensity of normal-scientific research, the paradigm is extended as far as it can fruitfully go. Thereafter, anomalous results and phenomena become widespread, and are recognized as genuinely problematic because they occur where the paradigm's guiding force should be most reliable, that is in the investigations which are in the mainstream of normal science. The recognition of the central significance of these anomalies provokes a crisis in scientific research. In a period of crisis, attention is focused on the anomalies. Attempts are made to fit them into the paradigm-based theory by patching up the latter with ad hoc additions. Ad hoc additions to theory cannot command general allegiance however, unless they lose their ad hoc character by being promulgated throughout the theory. In addition to patching up operations, therefore, there are both attempts to account for the anomalies with whatever conceptual schemes are available, and also searching critical examination of the conceptual scheme associated with the paradigm. The paradigm itself comes to be severely questioned.

Finally, an important set of the anomalous problems is accounted for by a new scientific achievement that has little contact with the principles and concepts related to the old paradigm. This achievement ushers in a scientific revolution, in which the old paradigm is overthrown and a new one replaces it. Insofar as the new achievement accounts for important anomalies and also seems general enough that it can account for many of the successes of the old paradigm, and insofar as the crisis state of the discipline has relaxed adherence

to the old paradigm sufficiently that scientists can consider alternatives to it, to that extent the new achievement may be able to form the next paradigm.

Once the new paradigm has become established, normal science can begin once again. The field has become significantly different however. The new paradigm achievement has its own implications for conceptual structure, problem selection, methodology, etc. In consequence, there is never complete continuity or mutual comprehensibility between successive eras of normal science.

Palermo's Kuhnian Analysis of Psychology.

Palermo uses Kuhn's analysis to interpret the careers of both Wundtian structuralism and behaviourism. As a basis for definition of the concept of a paradigm, Palermo focuses on its secondary or derivative usage, originally abstracted from the concept of a paradigm as a concrete scientific achievement (see pp. 33-34 above). This choice of focus, as will be seen below, is crucial to his analysis; it directs attention to the methodology and conceptual framework of a field as primarily constitutive of the paradigm, rather than as derivative of the initial exemplary achievement. Thus, in Palermo's formulation, a paradigm is

...the consensually agreed upon modus operandi of a mature scientific discipline. It consists of the conceptions of the nature of the theory to be used in guiding research, the types of problems worthy of investigation, the research methods appropriate to investigating those problems, and even, on occasion, the instrumentation which is required. These conceptions...determine the way in which the world of that discipline is viewed, and make it difficult for alternative conceptions to be considered (Palermo, 1971, p. 136).

Wundtian structuralism, according to Palermo, was psychology's first paradigm, and thus marked the emergence of experimental psychology as a mature science. Consistent with his interpretation

of a paradigm as the "modus operandi" of a science, Palermo sees the introspective method as central to the structuralist paradigm. The introspective method in turn determined the scope of experimental psychology, by excluding animals, children, and the insane from study. The normal science associated with the Wundtian paradigm consisted in the detailed introspective experiments in the laboratories of Wundt, Titchener, and their associates.

This structuralist paradigm entered a crisis state as a result of the unreliability of the introspective method (i.e., the lack of replicability of experimental findings across laboratories), and the inappropriateness of that method for the study of animal behaviour, mental illness, and developmental and educational psychology --all of considerable interest at the time, particularly to American psychologists. It should be noted, however, that all of these factors except the first are external to the structuralist paradigm. The crisis engendered a revolution sparked off by Watson's (1913a) famous polemic, "On psychology as the behaviorist views it", which abjured the introspective method and the study of consciousness in favour of objective methods for the study of observable behaviour. The revolution ended with the general adoption throughout American psychology of the behaviourist method and orientation toward psychology.

As Palermo describes it, the behaviourist paradigm had five main components, which together defined the common framework for the practice of experimental psychology. The first was the choice of subject matter and problems; the study of learning constituted the dominant subject matter of the field, almost entirely through the methods of classical and instrumental conditioning. Second was a concentration on simple forms of behaviour as the experimental preliminary to study of, and as the actual basis for, more complex behaviour.

The argument runs as follows: behaviour is learned; the simplest form of learning is conditioning; all other complex laws of learning derive from conditioning; once we know the basic laws pertaining to simple conditioning, we will be able to study more and more complex forms of behaviour which will involve the laws of conditioning related by various composition rules; the composition rules will be determined once the simple laws are known (ibid., p. 144).

Third was a related but less consistently applied emphasis on animal studies, that is on the study of simpler organisms as a preliminary to study of more complex ones, eventually including man. Fourth was a commitment to a position of anti-emergentism.

The behaviour of human organisms was considered basically no different from that of the chimpanzee, the dog, or the rat. It was accepted that the behaviour of these various species differs in complexity, but not in terms of the basic underlying mechanisms (ibid., p. 145).

Fifth was a commitment to a position of anti-nativism, or more positively to environmentalism--a belief that hereditary influences on behaviour were relatively unimportant and therefore that "Primary attention should be directed towards the effects of environmental factors in the determination of behaviour (ibid., p. 145)."

Palermo briefly mentions two other characteristics of the behaviourist paradigm which in part serve to sum up the five previous ones. First is that behaviourism was an S-R psychology: "The laws of behaviour were those which showed the relationships between variations in stimulus input and variations in response output (ibid., p. 146)."

Second, and as a result, any autonomous or stimulus-independent character of the organism or of its behaviour could safely be minimized.

...the organism is a passive receiver of stimuli which produce in a mechanical fashion, particular responses determined by the past history of the organism...The concept of motivation provided for an active organism, but the particular actions were determined by the stimuli present and by the past learning of the organism (ibid., p. 146).

Palermo mentions only in passing the normal science tradition within behaviourism, but affirms that it exhibited the cumulative and progressive pattern described by Kuhn.

The theory of learning was elaborated from the simple classical conditioning model from which it began. Significant variables were isolated and related in mathematical forms to characteristics of the response. The theory was extended, at first gradually, to related problems such as transposition and then, more broadly, to new areas such as social behaviour and personality (ibid., p. 146).

But as could be expected, serious anomalies eventually began to appear in the mainstream of behaviourist normal science. Palermo cites as the first serious anomaly a study by Kuenne (1946), in which she found that the well-developed Hullian theory of transposition could not account for significant parts of transposition behaviour in human children. Theoretical attempts to resolve this anomaly led to a good deal of subsequent research, but were of an increasingly ad hoc character and served to diminish the customary precision of behaviourist theorizing. Additional anomalies soon began to appear in diverse areas, in sufficient numbers that the paradigm began to come apart at the seams. Palermo cites the research of Harlow (1949, 1953, 1958) on learning sets, curiosity, and love, of Gibson (1969) on perceptual learning, of Bower (1967) on innate visual organization in infants, of Olds (Olds & Milner, 1954) on brain stimulation, of Rock (1957) and Estes (Estes, Hopkins, & Crothers, 1960) on one trial learning, and of many others as jointly causing psychologists "to become disillusioned with the adequacy of the paradigm (ibid., p. 149)."

However, Palermo feels that the most serious problems for the behaviourist paradigm come from Chomskyan psycholinguistics. Chomsky's work (e.g., Chomsky, 1957, 1965) first occasioned a revolu-

tion and paradigm shift within the already crisis-ridden field of linguistics, "changing that science from the behaviourist paradigm structured by Bloomfield to a rationalist approach with a transformational generative theory applied to the newly defined subject matter of that field (ibid., p. 151-152)." The implications of Chomsky's work concern psychology as well as linguistics, and indeed Chomsky feels that linguistics is properly a part, although by no means a subordinate part, of psychology.

There are two features of Chomsky's work that are of particular relevance to and significance for psychology. The first, and the more basic, is Chomsky's emphasis on the open-endedness or 'creativity' of language use. As one of Chomsky's expositors explains,

By this is meant the capacity that all native speakers of a language have to produce and understand an indefinitely large number of sentences that they have never heard before, and which may indeed never have been uttered before by anyone. The native speaker's 'creative' command of his language, it should be noted, is in normal circumstances unconscious and unreflecting. He is generally unaware of applying any grammatical rules or systematic principles of formation when he constructs either new sentences or sentences he has previously encountered. And yet the sentences that he utters will generally be accepted by other native speakers of the language as correct and will be understood by them (Lyons, 1970, pp. 24-25).

Since the range of sentences generated in a language is open-ended in this way, language use is not entirely under stimulus control. It follows that, even in principle, "S-R analyses of language behaviour can never adequately account for the acquisition and maintenance of language (Palermo, 1971, p.152)." Thus, Chomsky's emphasis on the creativity of language use both implies a severe critique of behaviourist models of language, and indicates the kind of alternative, less deterministic model which is necessary.

The second relevant feature of Chomsky's account stems

from the general mentalistic features of the alternative model which he provides. If it is accepted that language use cannot be accounted for solely in terms of stimulus control, then an analysis of language development and use must incorporate an analysis of the formal structures within the organism which are responsible for such language development and use. In this way, an analysis of language necessitates an analysis of mind.

The endeavour of the linguist provides the abstract characteristics of the mind of a person who speaks the language. The characterization of the language gives a characterization of the human who speaks the language. A person who speaks and understands the language in some sense knows the structure of the language which makes it possible for him to comprehend and use the language. It is in this sense that Chomsky is mentalistic (ibid., p. 152).

These characteristics of Chomsky's programme, Palermo suggests, may make it suitable to become psychology's next paradigm. They make contact with many of the points of greatest current dissatisfaction with behaviourism--particularly with concern over the appropriateness of S-R formulations, passive organism models, anti-nativism, and anti-emergentism. It is an open question as to whether Chomsky's approach will in fact find such widespread applicability, but so far at least it seems to be the best candidate for a sorely needed new paradigm in psychology.

Criticism of Palermo's Analysis.

Palermo's analysis makes an impressive fit to many of the events and trends associated with behaviourism, and thus has at least a strong prima facie plausibility. Nevertheless, it has received sharp criticism in three articles, somewhat overlapping in their critical content, by Warren (1971), Briskman (1972), and Mackenzie (1972). All three maintain that behaviourism was not, or did not have, a para-

digm, and that behaviourist research when it was dominant could not be considered normal science in anything like Kuhn's sense. The reasons for the objections were somewhat different, although in part again complementary, and will be considered individually (the last article listed will be covered in the next section).

Warren: behaviourism as school. Warren (1971) emphasizes that behaviourism could be called a paradigm, if at all, only by arbitrarily restricting attention to those parts of psychology in which it was in fact dominant. Behaviourism, that is, succeeded only where it was successful. Those parts of psychology in which behaviourism was successful never comprised all of psychology, and can be separated from the rest, Warren maintains, only by a process of post hoc selection abetted by parochialism. Behaviourism never provided a paradigm for Freud, Rorschach, Janet, or Jung; for Spearman, Binet, or Piaget; for German dynamic psychology or Gestalt psychology: it triumphed only in the U.S.A. Even there, behaviourism never became promulgated throughout the whole of psychology. Gestalt psychology, psychoanalysis, and most of clinical and factor analytic psychology thrived in America independently of and frequently in opposition to behaviourism. These psychological movements often disagreed violently with behaviourism, and among themselves, about what problems psychology should investigate, how they should be investigated, and in general what was to count as a scientific explanation in psychology. Insofar as a paradigm-based science has reached agreement on such fundamental issues, then behaviourism never provided a paradigm for all of psychology. Warren concludes that the Kuhnian analysis most appropriate to behaviourism and its competitors is that of competing schools in the pre-paradigm stages of scientific development. This conclusion

reflects Kuhn's own views, at least in his original treatise, on the status of psychology in this century (see footnote 1 to this chapter).

While Warren's point is well taken, it is not clear how much weight it should be given. The attitude of behaviourists towards many other approaches in psychology was often, at its most charitable, similar to Wundt's judgment of James' Principles: "It is literature, it is beautiful, but it is not psychology (Steffens, 1931; 1958 ed., p. 124)." For the behaviourists, as for Wundt, real psychology was scientific psychology. Non-behaviourist (like non-Wundtian) approaches to psychology might have many virtues, but being scientific was not usually one of them. Behaviourism was explicitly an attempt to recast psychology along what its proponents regarded as rigorously scientific lines. The fact that many persons who called themselves psychologists chose not to join in the attempt does not itself militate against behaviourism's paradigmatic status.

The achievement of paradigmatic status in a science, as Kuhn describes it, serves to differentiate that science from its diffuse background, not to assimilate the background completely into the new orientation. For a science to be acknowledged as having a paradigm, it is not necessary that everybody flock to the new banner. What is necessary is simply that there be a new banner, that it stand for something positive, and that an enduring group of scientists flock to it. After all, in other fields besides psychology, philosophical and variant empirical approaches continue to be operative even after a dominant paradigm has been established. Such variant approaches however, unlike one based on a paradigm, remain tied to their particular foundations and do not make continual, cumulative progress⁴.

Thus, the question of whether behaviourism is best regarded as a paradigm-based science or as a school is not settled by the fact

that there were many non-behaviourist psychologists. What is necessary instead is to examine the relationship between behaviourism and the other approaches. When this is done, it is clear that behaviourism was not merely one school among many, or even a primus inter pares. Rather, behaviourism to a considerable extent established the scientific standards, the level of aspiration, and the model of rigorous scientific theories for all psychology that attempted to have any scientific credibility (see Mackenzie & Mackenzie, 1974, for elaboration of this point). Behaviourism's putatively paradigmatic status, therefore, is not critically affected by the existence of non-adherents (although the scope of its paradigm is, as Palermo acknowledged). Questioning of that status must concentrate on the foundation and the scientific practice of behaviourism itself.

Briskman: behaviourism as research programme. Briskman (1972) shares some of Warren's reservations concerning the scope of behaviourism, but focuses attention primarily on the characteristics of behaviourism itself. Even within behaviourism, he argues, there was nothing corresponding to a paradigm or to normal science. Briskman maintains that the components of the behaviourist paradigm described by Palermo (see pp. 38ff.above) were variously methodological strictures and metaphysical beliefs. The methodological strictures "specify how one ought to go about the business of psychology; they tell one, in rough outline, how one is to proceed (Briskman, 1972, p. 91)." The metaphysical beliefs amount to "a faith that certain types of theories will be capable of accounting for all behaviour, from the simple to the complex, but [they are] certainly not powerful enough to actually generate those theories (ibid., p. 91)." Since the metaphysical beliefs were insufficiently powerful to generate any

specific theories⁵, the theories eventually developed within behaviourism were at wide variance with each other, and typically disagreed over fundamental issues concerning what is learned and how learning occurs. Much of behaviourist research in the period which Palermo felt characterized behaviourist normal science was directed, not to articulation of a paradigm (as in normal science), but to the refutation or discrediting of rival theories. Thus, Briskman argues, if behaviourism was a paradigm, it must have been in a crisis state from the beginning. Briskman maintains that behaviourism is best considered instead as a "methodological-cum-metaphysical research programme (ibid., p. 92)"--a concept adapted from Lakatos' (1968, 1970) general analysis of scientific progress--that has degenerated, or become less informative or generally viable, as a result of repeated refutations of specific behaviourist theories.

Briskman's point about the dubiousness of the status of behaviourism's paradigm is a good one. Unless the criteria for attribution of a paradigm are explicated more clearly than they are by Palermo, and unless the content of behaviourism's particular paradigm is described more specifically, then it is difficult to decide whether the characteristics of behaviourism as described by Palermo should be related to a paradigm, a research programme, or something else again. Furthermore, Briskman's contrast between typical behaviourist research and Kuhnian normal science is an illuminating one. Even in the absence of a clear definition of a 'paradigm', this contrast reveals a difficulty in applying a Kuhnian analysis to psychology (both of these points will be further developed below).

Unfortunately, Briskman's analysis has in some respects the same weakness as Palermo's, in that he does not make clear the meaning

of a 'paradigm' or of a 'research programme', or describe in sufficient detail the differences between them. He obviously regards the two as significantly different, but does not systematically contrast them to show where their differences lie. Indeed, on the basis of his description alone, it is difficult to see just where they might lie. Half of Briskman's research programmes consist of methodological strictures, and Palermo explicitly regards methodological considerations as central to both the behaviourist and the structuralist paradigms. The other half of his research programmes consist of metaphysical beliefs, and these, too, seem on most interpretations entirely compatible with the set of loose implications typically derived from a paradigm⁶. Metaphysical beliefs, for instance, make up one of the three classes of use of the term 'paradigm' discovered by Masterman (1970) in her textual analysis of Kuhn (cf. footnote 3 to this chapter).

Briskman indicates at some points that his conception of research programmes is intended to apply to all scientific activity, as is certainly the case with the original from which his conception is adapted. If so, then research programmes must constitute a general alternative to Kuhnian paradigms as an account of the basis of science. Otherwise, Briskman would be constrained to show why research programmes and paradigms are significantly divergent in psychology but not elsewhere. Certainly, criticisms of and alternatives to Kuhn's account of scientific activity are germane to an attempt to understand both the history and the present practice of science. But Briskman does not document his case for science in general, and gives no account of his proffered alternative except in the context of behaviourism. In behaviourism at least, he says, there was no paradigm; instead there was a research programme. He does not explicitly consider whether there

are paradigms anywhere, but implies at least that there are research programmes everywhere in science.

As a result, it is "everywhere in science" that the validity of the conception of research programmes needs to be assessed, and not specifically in psychology. Briskman's error lies in presenting as an alternative to Palermo an account which makes sense only as a general alternative to Kuhn, while offering evidence for his account only within psychology, where in isolation it cannot be adequately assessed. It cannot be adequately assessed there, partly because any such general conception needs to be tested in a wider context, but partly also because the general question of the relative merits of paradigms and research programmes in the rational reconstruction of science is not entirely relevant here. Psychology is a special case, purposefully not covered in Kuhn's original analysis, in which the question at issue is precisely whether a Kuhnian analysis is as applicable as it is to other sciences. Briskman does not address himself to this question. His contention that a Kuhnian analysis is inapplicable to psychology is based on a model of scientific advance which, if applied consistently, would seek to show the insufficient applicability of a Kuhnian analysis everywhere⁷. But Briskman presents no evidence for the latter position, and even within psychology does not differentiate his model sufficiently from the Kuhnian one that a clear choice can be made between them.

II. Behaviourism and Methodological Objectivism.

Palermo and Briskman agree in principle that the same analysis can be applied to the development both of psychology and of the natural sciences; their disagreement is over what that analysis should be. By contrast, I would claim along with Kuhn (1962), although for different reasons from his, that the development of psychology has

been systematically different from that of the natural sciences, and that the same analysis cannot therefore be applied to both. In this section I will begin to develop an alternative historical and critical analysis of behaviourism, one which I believe does greater justice to the circumstances of its founding, the spirit of its most mature formulations, and the pattern displayed in its decline. First, I will explicate further the notion of a paradigm, and attempt to show how the notion cannot profitably be applied to behaviourism. Next, I will develop a 'low level' descriptive account of the objectivist framework which I hold served as the basis for behaviourism, and show how the course of subsequent research based on this framework can be differentiated from research based on a paradigm. This descriptive account will serve as the basis for a 'higher level', more strongly interpretive account of the foundations of behaviourism, which will be developed in succeeding chapters.

The Content and Concreteness of Paradigms.

In an article (Mackenzie, 1972) that began with a short discussion of Palermo's position⁸, I agreed that behaviourism was, in much the way that Palermo describes, the dominant movement in American experimental psychology. Since it had a clear and self-consciously revolutionary beginning, and at least a certain amount of cohesiveness, there is some apparent justice in attributing to it a Kuhnian paradigm. However, while behaviourism displayed certain of the features normally derivative of a paradigm, it did not have the paradigm itself⁹; and this lack rendered its subsequent development quite different from the expected pattern of normal science.

The point to be made here was discussed very briefly in the article referred to, where it received some criticism for its lack of

clarity and detail. Therefore, it will be developed in more detail here; it will require further consideration of the notion of a paradigm. In contrast to Palermo, I wish to focus attention on Kuhn's initial meaning of 'paradigm' as a concrete scientific achievement, and to show that this is a root meaning indispensable to any further elaboration of the concept.

Whether one accepts that a paradigm must be based on a concrete scientific achievement or not, it will not be denied that in Kuhn's analysis each paradigm which he considered was in fact based on such an achievement. Examination of the way in which such achievements come to have the status of paradigms indicates that the concrete achievement itself, including its specific content and its substantive significance in its historical context, is necessary to the composition of a paradigm. Other bases for achieving a "consensually agreed upon modus operandi" may certainly exist, but unless it was clearly shown how they could have the same implications for the practice of science as a paradigm would, they could not legitimately be assumed to do so.

When it is first advanced, the scientific achievement that eventually becomes a paradigm is a major substantive contribution to knowledge in the discipline in which it appears. Its initial importance is that it has solved a problem that was generally recognized throughout the discipline as both important and intractable; it was the existence of such generally recognized problems that put the previous paradigm-based tradition into a crisis state. This 'paradigm achievement' serves as the basis for a new scientific tradition by stimulating the development of a new conceptual framework, a new set of problems (or rather puzzles) to solve, a new methodology for solving them, etc. The connection between all of these metascientific develop-

ments is that they are all justifiable by reference to the paradigm achievement in which they were, in preliminary form, first embodied or suggested.

This point can be extended. A paradigm achievement is initially advanced in the context of a period of crisis affecting the previous paradigm-based tradition. Thus, the paradigm achievement is both prior to and partly independent of the subsequent tradition which it inaugurates. It is incorporated into that tradition as an exemplar, but it makes sense, that is it can be examined and assessed as significant, outside of its subsequent incorporation. The context which is necessary for recognition of the significance of the achievement is a minimal one; it corresponds to the comparative anarchy of theoretical and conceptual frameworks that is typical of scientific research during a crisis state. While there are of course still standards for the assessment of scientific achievements during a crisis, these standards are not nearly so theory-dependent or paradigm-linked as they are during a period of normal science. A major scientific achievement accomplished during a period of crisis thus has greater generality of application than does one accomplished within the context of normal science.

The important point in this is that the substantive significance of the paradigm achievement is such that it can be recognized and referred to as justification for later research based on it, independent of the standards for the conduct of such research subsequently derived from the achievement itself. Normal science is justified by its relation to the paradigm, but the paradigm achievement must be capable of justification in some other way, that is, by criteria other than those which it newly establishes. Thus, there are some standards



for evaluation of scientific work which are paradigm-independent, or at least relatively so, and which come to be emphasized when the paradigm-derived standards are in question (cf. below, footnote 10). It is for this reason that there is at least some continuity and cumulativeness in science, even across revolutions. It is for the same reason that Kuhn describes communication across revolutions as being partial, rather than nonexistent (as some of his critics interpret him to claim) or total (as some others might wish to claim).

To recapitulate, the paradigm achievement can be specified and examined as existing distinct from the subsequent tradition which incorporates it as an exemplar. It is independent of that tradition both through its temporal priority and through its autonomous substantive merit¹⁰. These two factors make the paradigm achievement able to provide justification, based on its already demonstrated success, for all of the developments in methodology, problem selection, and conceptual framework that are abstracted from it. Conversely, in a new paradigm-based tradition, only those developments in methodology, etc., occur, which can be abstracted from and justified by reference to the paradigm achievement.

These considerations make it clear that the content and the substantive significance of the paradigm achievement are central to the constitution of that achievement as a paradigm. The content of the achievement is what stimulates and directs further scientific activity, and provides the basis for consensus on what research is to be done and how. The substantive significance of the achievement is what draws attention to the achievement and provides justification for making it the basis for further practice. These two features--direction and justification--comprise the functional significance

of the paradigm in the course of science. It is on their account that the postulation of 'paradigms' as the basis of science can be claimed to be more informative about the process of scientific activity than can the postulation of alternative, more general methodological or epistemological conceptions, such as 'objectivity' or 'falsification'.

In short, the functional significance of a paradigm is based upon its content and upon its substantive significance. It is for this reason that I reject Palermo's characterization of a paradigm as "the consensually agreed upon modus operandi of a mature scientific discipline." Palermo affords the paradigm neither any defining substantive content nor, following from this, any possibility of an independent directive influence over the tradition which supposedly makes the paradigm the basis for its further practice. Instead, for Palermo the paradigm functions only as embodied in the ongoing course of scientific activity, in the form of what I have described as the developments in methodology, problem selection, etc., stimulated and justified by it. Hence, for Palermo the paradigm itself reduces to an abstraction from whatever pattern can be discerned in scientific activity, and it is impossible to distinguish methodological and other developments which are based on a paradigm from those based on something else. Indeed, on Palermo's account there could be by definition nothing else on which they were based. His conception of a paradigm would allow any organized or systematic activity to claim a paradigm as long as it has a "consensually agreed upon modus operandi." This minimal version of a paradigm could not be assumed to have the functional significance attributed to paradigms by both Palermo and Kuhn. For that purpose a stronger version is required. Palermo's

conception, for instance, would not make it possible to distinguish between a mature, paradigm-based science, and one of a number of competing schools in the pre-paradigm stage of scientific development (in Kuhn's thesis). It is certainly conceivable that something other than a paradigm as concrete achievement could have a similar functional significance, but any such alternative would require a detailed description and demonstration which Palermo does not provide.

Palermo's restriction of his characterization of a paradigm to "mature scientific disciplines" is of little help in limiting its diffuse applicability, because he does not give any independent criteria of what constitutes a mature science. The term is taken from Kuhn's treatise, but there Kuhn means by a mature science only a science that displays the pattern of scientific activity associated with dependence on a paradigm. This characterization of a mature science is trivial if 'paradigm' is used in Palermo's sense, but not if used in Kuhn's root sense of a major scientific achievement¹¹.

In the stronger or more restricted sense in which I will henceforth use the term then, a paradigm may be characterized as a major scientific achievement that comes to serve as an exemplary source of principles, concepts, methods, problems, and 'goggles' (or conceptual frameworks) for practitioners in a scientific discipline. This characterization accords well with Kuhn's original basic use of the term, and I have indicated why this sense must be taken as fundamental. Making it explicit serves to eliminate, at least in the present context, much of the ambiguity that has surrounded the notion of a paradigm¹².

Methodological Objectivism as the Basis of Behaviourism.

Now, it is clear that behaviourism never had a paradigm in this sense. That is, it was not based on or initiated by one or a

linked set of scientific achievements of demonstrated and continuing substantive significance for psychology. Instead, it was based on a dissatisfaction with the way that psychology was being done by those who still dominated the field, and on a promise that a new way of doing things would bring important and scientifically respectable results.

There are three publications or researches that can be taken jointly as the foundation of behaviourism: Thorndike's early animal researches, Pavlov's investigation of the conditioned reflex, and Watson's polemical announcement. This is not to say that these three provided a complete basis for behaviourism, but that insofar as behaviourism was founded on specific achievements in psychology, these were the ones. The first two provided the models for almost all behaviourist experimentation on learning--the models of instrumental (Thorndikian, operant) conditioning and classical (Pavlovian, respondent) conditioning. The last signalled the start of behaviourism as a self-conscious movement. None of these three, however, constituted a major and substantive scientific achievement, at least not for behaviourism.

The qualification, "at least not for behaviourism", is important. Both Thorndike's and Pavlov's early researches were addressed to important questions in their respective fields and made some progress in answering them. Thorndike (1898) attempted to demonstrate that, in learning to escape from puzzle boxes, his subject animals acquired their responses solely on a basis of trial and error. He advanced the major substantive hypothesis that all learning might conform to this pattern displayed by animals in his experimental situation, that is that all learning might proceed by trial and error without any

efficacious exercise of insight or conscious reflection. In proposing this hypothesis, he was setting himself explicitly in opposition to the broadly cognitive hypothesis of Romanes (1882) and Lloyd Morgan (1894). His use of puzzle boxes was a relevant and useful method of experimentation precisely because of the new light which it was able to throw on the contentious current issue which he was investigating.

However, Thorndike's major hypothesis and the evidence on which it was based--which might have constituted a paradigm in the strong sense--did not in fact constitute an agreed upon basis for behaviourism. On the contrary, it was the source of one of the most intensive theoretical controversies for behaviourism. Lashley, Krechevsky, and Tolman (to cite some of the major figures only) maintained that some sort of insight or hypothesis testing or central representation--these terms defined objectively of course--had to be attributed to organisms in a learning situation in order to account for their learning. Their position was almost the precise opposite of Thorndike's, and was much the same position that Thorndike had been attempting to discredit in its earlier less objective formulations by Romanes and Lloyd Morgan. In contrast, Watson, Guthrie, Hull, and Skinner agreed that no such attribution of insight, etc., was necessary or relevant to an account of the learning process. They disagreed among themselves otherwise, and Watson and Guthrie, at least, insisted that 'trial and error' could no more serve as a first approximation to a description of what went on in learning than could 'insight'. In short, behaviouristic psychologists as a group never agreed on acceptance of Thorndike's results and his systematic hypotheses as the basis for their subsequent practice. What they did agree on, and almost all

that they agreed on, was that Thorndike's use of puzzle boxes was a good way to answer questions about animal behaviour. They agreed that it was a good way, not primarily because Thorndike's experimental technique was finely suited to the problems which he was investigating, but because it was a reliable and objective method for the study of behaviour. It is noteworthy that this agreement on the worth of Thorndike's method persisted despite the fact that it never proved sufficient to secure any agreement on the answers themselves.

The case with Pavlov is similar but even more pointed, for the theory and the problems to which Pavlov's method was connected never made a major entry to behaviourist psychology at all. Originally, the conditioned reflex was discovered and used by Pavlov in the study of digestive processes. Its properties were sufficiently remarkable that Pavlov and his co-workers began to give it very serious consideration in its own right, and came to use it as the basis for study of higher nervous activity (e.g., Pavlov 1927). The conditioned reflex was not treated by them as a tool, separate from the problems which it was used to investigate, but as a fundamental manifestation of nervous excitation. In investigating the conditioned reflex, they felt that they were necessarily investigating nervous activity. As a result, for Russian workers use of the conditioned reflex was inseparable from Pavlov's theory of nervous excitation and inhibition. This coalescence of Pavlovian theory and method, each dependent on and supporting the other, has continued to be typical of Russian psychology, in which a vigorous theoretical and experimental programme is still extending Pavlovian theory to cover more and more aspects of human behaviour. It is thus likely that a Kuhnian analysis could be profitably applied to the development of Russian psychology after

Pavlov, for Pavlov's early work would seem to constitute a paradigm, and subsequent Russian psychology was certainly based on it¹³.

By contrast, in behaviourist psychology, the only acknowledged and enduring merit of the conditioned reflex, which was nevertheless sufficient to stimulate its widespread experimental use, was that it was an objective method for the study of behaviour. The Pavlovian theory of cortical excitation was regarded by most American psychologists, and continues to be regarded, as either preposterous or, at best, ill-founded. As with Thorndike's method of animal experimentation, the conditioned reflex received general approval predicated almost solely upon its objectivity, and hardly at all upon its presumed unique appropriateness for answering certain specific questions asked in certain specific ways. The use and extension of Pavlov's method of classical conditioning, like the use and extension of Thorndike's method of instrumental conditioning, has been piecemeal, general, and not closely related to a specific theoretical matrix.

Finally, Watson's polemic was not intended to advance seriously any specific hypotheses, nor even primarily to advance any specific methods as appropriate for the study of then current problems. Rather, its positive intent was to stipulate that psychology could and should be exclusively the objective study of observable behaviour, to maintain that a certain class of methods--those which were objective or which abjured any form of mentalism--were the appropriate ones for studying such behaviour, and to argue that psychology as so conceived and practiced would become a successful scientific enterprise.

In summary, two of the founding achievements of behaviourism had considerable substantive significance in their own context, but in the form in which they were incorporated into behaviourism they were

shorn of their content and adopted solely for their methodological import, that is, for their objectivity. The third, Watson's polemic, made little in the way of substantive claims at all but merely advanced the cause of objectivity per se. Thus, none of these three achievements answered any major questions--for behaviourism--or solved any longstanding problems in psychology--for behaviourism. Instead, they were taken by behaviourist psychologists to proclaim that human and animal functioning could be understood in a particular way (the way of objective experimentation), and to promise that the use of a properly objective methodology would make psychology into a genuine science. It is for this reason that Thorndike's and Pavlov's results were never of principle importance; instead it was their techniques, and the methodological principles abstracted from their techniques, that were valued and that became central. The revolution that produced behaviourism was a methodological, or even a meta-methodological, revolution.

The foundation of behaviourism may thus be characterized at least in part as methodological objectivism, that is, as the pursuit of objectivity through the exclusive employment of methods which were themselves known to be objective. This pursuit of and emphasis on objectivity undoubtedly filled a need that was felt in American psychology at the time. As Boring explains it, "Psychology was all ready for behaviourism...the times were ripe for more objectivity in psychology, and Watson was the agent of the times (Boring, 1950a, p. 642)."

But just what is this touchstone of 'objectivity' by which experimental techniques were assessed? Why, when it was discovered in them, did it suffice for their adoption independent of any regard for the theoretical context in which they were developed and embedded? These may seem like odd questions; surely 'objectivity', in a minimal

sense that implies an absence of bias and a concern with observable events, whatever they might be, is basic to any scientific enterprise. And perhaps it is, but this minimal sense of 'objectivity' does not differentiate behaviourism from Wundtian structuralism. In this minimal sense, the introspective method of Wundt and Titchener also was, or was at least intended to be, thoroughly objective. Watson acknowledged at least this intended character of Wundt's programme in stating that "Wundt, the real father of experimental psychology, unquestionably wanted in 1879 a scientific psychology (Watson, 1924; 1961 ed., p. 3)."

The introspective method was judged to be unreliable of course, since results obtained with it were not always replicable at different laboratories, and this unreliability provided justification for rejecting it. Equally, the desire to use methods that had high inter-experimenter reliability was a factor in the adoption of Pavlov's and Thorndike's methods. However, the question of the objectivity of these methods was not subordinate to the question of their reliability; instead the converse was true. Watson was quite clear that the introspective method was unreliable because of the crippling flaws inherent in it from the beginning. The lack of objectivity in that method was not simply discovered as a consequence of its unreliability; rather its unreliability might have been predicted in advance because of its lack of objectivity. Again, when Watson first advocated use of the conditioned reflex for the study of behaviour (Watson, 1916), that method had been used in a few preliminary studies in Watson's own laboratory, but otherwise had received almost no trials or applications outside of Pavlov's laboratory. It had thus not been shown to have precisely that inter-experimenter reliability, the absence of which

was the ostensible justification for rejecting introspection. The objectivity of Pavlov's method, nevertheless, provided a sufficient guarantee that it would be reliable, and made experimental confirmation of its reliability not really necessary¹⁴.

Obviously, therefore, the objectivity which behaviourism required, and which it found in the methods of Thorndike and Pavlov, was something other than or additional to the objectivity that was supposed to be (even if it was not sufficiently) characteristic of Wundt's method. The additional element lay, on the one hand, simply in the rejection in principle of any concern with mental events, on the basis that these were, by their very (alleged) nature, impossible to investigate objectively. The failure of the introspective method was due simply to the fact that it was attempting to investigate something that could not be investigated. Thus, Watson characterized introspection and its downfall as follows:

[Wundt] grew up in the midst of a dualistic philosophy of the most pronounced type. He could not see his way clear to a solution of the mind-body problem. His psychology, which has reigned supreme to the present day, is necessarily a compromise. He substituted the term consciousness for the term soul. Consciousness is not quite so unobservable as soul. We observe it by peeking in suddenly and catching it unawares as it were (introspection)... In 1912 the objective psychologists or behaviorists reached the conclusion that they could no longer be content to work with Wundt's formulations. They felt that the 30 odd barren years since the establishment of Wundt's laboratory had proved conclusively that the so-called introspective psychology of Germany was founded upon wrong hypotheses--that no psychology which included the religious mind-body problem could ever arrive at verifiable conclusions (Watson, 1924; 1961 ed., pp. 3-5).

Both points are brought out clearly in this quotation: that Wundt's programme was scientifically sterile from the beginning, and that its sterility was a necessary result of its concentration on mind and consciousness.

The objectivity which behaviourism sought was thus one which from the beginning excluded the mental from the domain of science, and did so independent of purely evidential considerations. This a priori character of the exclusion of the mental is sufficiently indicated both by the explanation given to the failure of introspectionism and by the reception afforded to Pavlov's method. The demonstration is reinforced, furthermore, by consideration of the specific issues which, more than anything else, are taken to have sparked off the behaviourist decision to repudiate introspection in general as both unreliable and untenable.

The most important of these issues was the 'imageless thought' controversy raging between Wundt's school and the Wurzburg school. This controversy, over whether or not thought could in principle be reduced to trains of images, occasioned a large amount of contradictory research and polemic on both sides. The dispute was never resolved altogether, although the Wurzburgers had somewhat the better of it. The dominant reaction to the controversy among American psychologists, at least those not involved in it, was that if the introspective method could not even begin to settle so apparently basic a question as to whether or not thought is made of images, then what good was it? The answer seemed plainly to be, not much. Boring writes that shortly before behaviourism was launched, "Watson had just seen introspection fail at Wurzburg--that is what most Americans in 1910 thought had happened there (Boring, 1950a, p. 642)."

However, the introspective method used at Wurzburg was very different from the one used in Wundt's laboratory; the two were related to quite different theories of mental functioning. That the results obtained with the two methods were not identical was no more a

methodological failure of introspectionism than the inability to obtain many responses equally well with both classical and instrumental conditioning is a methodological failure of behaviourism. Furthermore, the Wurzburg results were not merely the (possibly artifactual) product of a single laboratory. The imageless thought hypothesis was independently confirmed by Binet in France, Bovet in Switzerland, and Woodworth in America. The findings from Titchener's laboratory, against imageless thought, might however have been artifactual in this sense, as they were never independently confirmed at another laboratory (Wundt never performed any Wurzburg-type experiments)¹⁵.

This is not to underplay the importance of the imageless thought controversy. The dispute between the Wundtians and the Wurzburgers was a very major theoretical and experimental problem for both of them, and had it been resolved would probably have led to a serious modification of one or both methods and theories. But the dispute did not constitute a methodological failure generally of all introspective methods, at least not for introspectionists, including those who were uncommitted in the controversy. It could be seen as a general failure only by those who were already prepared to repudiate introspection on other grounds. The two introspectionist methods could be treated as equivalent, and their divergent results attributed to the unreliability of introspection in general, only by treating the differences between the two methods as insignificant; and that in turn could be done only by contrasting both methods indifferently with an already favoured alternative conception of scientific observation in which introspection of any sort is excluded from the beginning. In short, interpretation of the imageless thought controversy as showing the unreliability of 'the' introspective method, or of all of them, was

possible only within a framework which was already more narrowly objectivist or proto-behaviourist.

Neither, it may be worth mentioning, can the rejection of the mental be justified as some sort of extreme but inevitable reaction to a rigid introspectionist orthodoxy, as the only way in which 'objective'--non-introspective--methods could gain any hearing at all.

Introductory students sometimes gain the erroneous impression that the behaviourist revolution was necessary in order newly to establish non-introspective methods as permissible and appropriate in psychology; and they are not totally corrected of their error if they read Watson's own statements on the subject. In fact there was a wide diversity of methods in use in experimental psychology--particularly but not solely in human experimental psychology--before 1913; introspective and non-introspective methods of investigation were employed, side by side and independently, with greater flexibility and tolerance than has been characteristic of psychology since that date. Woodworth (1931), who was himself very active in the period, addresses himself to the question of the amount of objective or non-introspective research carried out before 1913. Prior to that date

...there had been a large amount of objective experimental work by those who were interested in what I have been calling the psychology of performance. Watson alludes to some of it [1914]...when he speaks of experimental pedagogy, the psychology of tests, etc. He leaves the impression that all such objective work was very recent, as well as being partially vitiated by introspection. Tests, completely objective and free from introspection, go back to Galton in about 1880. Objective study of the learning process was active in the nineties, and may be dated back to Ebbinghaus's celebrated study of memory in 1885, a purely objective study. But even Galton and Ebbinghaus are not entitled to rank as the originators of objective psychology, for still further back we find the purely objective beginnings of work on reaction time; and much of the work on sense perception, carried on by the method of impression, can perfectly well be considered as an objective study of an individual's accuracy of observation. Thus the objective method was in

use from the very beginnings of experimental psychology, and the amount of research carried on by this method, up to 1900 or 1912, was very large indeed. As we saw before psychology was not at all limited in practice to the study of experience. The study of performance was in full swing, and even those psychologists who made great use of introspection, like Müller and Külpe, used it largely for the light it threw on performance (Woodworth, 1931, p. 48).

During this period there were, of course, a great number of experiments carried out with introspective methods that might better have been conducted with non-introspective ones. The point is merely that non-introspective methods were firmly enough established in psychology that there was little in the way of entrenched social forces that could have been expected to prevent their further systematic application.

Again, therefore, behaviourism's initial and central rejection of the mental and exclusive concentration on objectivity cannot legitimately be justified, as they are often purported to be, by reference to the state of introspective psychology of the time--as being of demonstrated scientific worthlessness or as exercising an oppressive scientific hegemony. The behaviourist stance was taken largely in self-conscious opposition to introspective psychology of course, but not in response to the inadequacies of the latter. Instead, the factors responsible for the emergence of behaviourism were, as we shall see in Chapter 4, unrelated to most of the career of introspective psychology, and totally unrelated to the problems introspective psychology was facing at the time.

The negative characterization of behaviourism's brand of objectivity, that it was in principle anti-mentalistic, is an obvious and familiar point, even a trivial one. What is not so trivial is the demonstration that the 'principles' involved, and the grounds for the

adoption of an anti-mentalism, were a priori ones; or at least ones not dictated by experimental evidence. To sum up the point once more: 'objective psychologists' rejected introspectionism because they were anti-mentalistic; they did not become anti-mentalistic on the basis of having had to reject introspectionism.

The converse of the rejection of mentalism, and the affirmative side of behaviourism's quest for objectivity, was the acceptance of a loose 'physicalism'. Behaviourism's methodological objectivism was of a sort that related the methods and scope of psychology to those characteristic of the natural sciences. Wundt's distinction (also accepted by Titchener) between 'mediate experience' and 'immediate experience' as the basis for demarcation between physics and psychology was to be abolished¹⁶. The same logical methods, the same observational techniques, the same standards of evidence, and the same criteria of validity were to apply to psychology as to physics. Hence, the kinds of things that psychology was to study were to be at the most general level the same kinds of things as physics studied--bodies in motion. Applying this precept to psychology dictated a restriction of concern to publically observable behaviour. Thus, that behaviour alone is the proper subject matter for psychology was indicated by the example of successful sciences as well as by the example of what was defined as an unsuccessful one, Wundtian structuralism.

The insistence that the canons of science for psychology were to be the same as, and where necessary taken over from, those of the natural sciences was a feature of behaviourism from the beginning. As Watson put it, the behaviourists

decided either to give up psychology or else to make it a natural science. They saw their brother-scientists making progress in medicine, in chemistry, in physics. Every new discovery in those fields was of prime im-

portance; every new element isolated in one laboratory could be isolated in some other laboratory; each new element was immediately taken up in the warp and woof of science as a whole. One need only mention wireless, radium, insulin, thyroxin, to verify this. Elements so isolated and methods so formulated immediately began to function in human achievement (Watson, 1924; 1961 ed., p. 5).

The behaviorist asks: Why don't we make what we can observe the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws concerning only those things...You will find, then, the behaviorist working like any other scientist. His sole object is to gather facts about behavior--verify his data--subject them both to logic and to mathematics (the tools of every scientist) (ibid., p. 6.).

It may never make a pretense of being a system. Indeed systems in every scientific field are out of date. We collect our facts from observation. Now and then we select a group of facts and draw certain general conclusions about them. In a few years as new experimental data are gathered by better methods, even these tentative general conclusions have to be modified. Every scientific field, zoology, physiology, chemistry and physics, is more or less in a state of flux. Experimental technique, the accumulation of facts by that technique, occasional tentative consolidation of these facts into a theory or an hypothesis describe our procedure in science. Judged upon this basis, behaviorism is a true natural science (ibid., p. 18-19).

The tendency toward aggressive self-assimilation into the natural sciences remained much the same as behaviourism developed. Weiss (1925) went considerably farther even than did Watson, in a surprisingly detailed and sophisticated attempt to show that both the methods and the content of psychology could be formulated in terms appropriate to atomic physics. Eschewing Weiss' reductionism, Skinner was the first psychologist to concern himself with the recasting of psychology according to Bridgman's operationist principles (in 1929; see Skinner, 1945). Hull chose detailed examples from the history of physics and astronomy to provide illustrative examples of how science should be carried out (e.g., Hull, 1939).

It is relevant to note that the adoption of a natural-science based orientation in behaviourism was itself, strictly speaking, a priori, just as was the rejection of mentalism; although the former provided much of the basis for the latter. The rejection of mentalism was justified by appeal to the practices of physics, but the appropriateness of these practices for comprising the entire basis for psychology had by no means been demonstrated at the time when they were first advocated. However, the general validity of basing psychology in some way on the natural sciences, and particularly on physics, was an assumption that had rarely been questioned in any kind of empirically oriented psychology, or more generally in any which had claims to being scientific. Behaviourism, in searching for a lead from the natural sciences, was at least in this respect only following a respected tradition.

The Contentual Pluralism of Behaviourist Research.

The foundation of behaviourism, then, was an anti-mentalist methodological objectivism adopted largely on a priori grounds. Just what these grounds were will be discussed in detail in the following chapters. By way of preview they may be briefly mentioned here. The first, already partly discussed, involved the tendency endemic to psychology at least since Locke, to attempt to base psychology on the methods and models of the physical sciences; the advent of behaviourism was signalled by a new turn in this longstanding trend. The second was an entirely legitimate dissatisfaction with the way in which introspective psychology was closely associated with comparative psychology, a dissatisfaction that might have led quite reasonably to a separation of the two.

It remains to consider the character of the research based on the objectivist foundation of behaviourism. It should be kept in

mind that what differentiates the foundation of behaviourism from a paradigm is not merely that it was a priori, but that it was methodological and non-specific. The faith in the power of an objective methodology was not closely tied to any concrete achievement in psychology, nor even to a particular methodology. The faith was a general one, repeatedly affirmed on the basis of the promise which it issued for future success, and justified by appeal to other 'objective' sciences which were already successful. Keeping the faith, therefore, was fully compatible with the development of numerous diverse formulations of the scope, methods, and theoretical orientation most appropriate to psychology, with the different viewpoints connected by little more than their anti-mentalism and their pursuit of objectivity. And indeed, in just such a way did behaviourism develop. What was most vigorously 'articulated' in behaviourist research was this very objectivism, through the development of a multiplicity of methods and models for investigating behaviour in the newly objective manner.

There were two factors which were responsible for that degree of cohesiveness which existed in behaviourist research and theory. The first was the agreement on objectivism itself, an objectivism which had as its central tenet the repudiation of unobservable internal agents or substances. It implied, therefore, at the very least a severe distrust of alleged internal influences on behaviour. A partial exception could be made for physiological internal factors, since these were both physical and, in consequence, observable in principle. Even so, the fact that such physiological factors were usually not observable in practice precluded the attainment of any unanimity on the value of implicating them in psychological theories. Physiological references were admissible in behaviourist theories, but were not

crucial. Other internal factors received considerably shorter shrift. Instincts, desires, expectations, affects, etc., were afforded little credence unless they were reduced entirely to either physiological or behavioural measures.

The other factor accounting for some cohesiveness in behaviourist research was its continuity with pre-behaviourist animal psychology. That psychology, in both Britain and America, was based largely on Darwinian evolutionary theory, and was concerned with the adaptive capacities of behaviour as a reaction to changing external circumstances. While behaviourism gradually cast off almost all traces of a Darwinian orientation (see Chapter 4), the tradition of experimentation on animal learning was sufficiently well developed to continue as viable. Indeed, it was in the context of this tradition that the demand for greater objectivity emerged. This continuity was largely responsible for the original channelling of interest into problems of learning; and conversely, experimentation on learning was the context most suited to the newly defined or refined requirements of objectivity.

Within the limitations produced by these two factors, which were not very restrictive at least as far as the content of theories was concerned, behaviourist theories developed in wide divergence and often in basic conflict with each other. Watson's original programme was mechanistic, elementaristic, associationistic, peripheralistic, environmentalistic--and correspondingly anti-teleological, anti-purposive, anti-nativist, and anti-emergent. None of these features received full assent from later behaviourists.

Tolman's re-introduction of purpose into behaviourism took place only six years after Watson began promoting conditioning

principles (Tolman, 1922). In his fuller theory, Tolman (1932) continued to abjure mentalism as such, but made free use of cognitive concepts such as expectancy. In doing so, he repudiated elementarism by insisting on the primacy and irreducibility of molar behaviour, and minimized the implication of associationistic and mechanistic linkages between stimuli and responses by stressing the organism's selective control over its environment. Krechevsky, who studied under Tolman, continued in a similar vein, hypothesizing hypotheses in rats (e.g., Krechevsky, 1932). The theoretical concepts introduced by Tolman and Krechevsky were not formally mentalistic ones, despite their appearance, because they were classed strictly as intervening variables, with no assumed functional significance or existence apart from that which they openly displayed in behaviour¹⁷. Nevertheless, or rather as a result¹⁸, emphasis on such concepts made a break with the mechanistic features of Watson's programme inevitable.

Watson's peripheralism--which was, it is true, never unambiguously formulated--was shown to be wholly untenable by Cannon, in his The Wisdom of the Body (1932), a work which gave the conception of homeostatic mechanisms a wide currency among psychologists, and thereby re-introduced the inside of the body into psychology in the same year in which many felt that Tolman had re-introduced the mind. Hull made a detailed attempt to ground all of his major theoretical postulates in physiological mechanisms (e.g., Hull, 1943), an attempt later shelved by his student and colleague Spence (1950). Spence also hedged his bets, at least, with regard to the question of anti-emergentism, particularly as related to human language behaviour¹⁹.

Hull also accepted, with reservations, Tolman's emphasis on the molar characteristics of behaviour, and postulated mechanisms to

account for the teleological and purposive features of behaviour (these, however, did not constitute an admission of purpose as such, but were rather an attempt at an experimentally based translation of 'subjective' concepts into mechanistic 'objective' ones). In addition, Hull (1943) postulated a fairly long list of inborn drives, including drives for hunger, thirst, maternal behaviour, air, pain avoidance, maintenance of body temperature, defecation, micturition, rest, sleep, and activity, and judged that additional ones might well prove necessary. While not all of these drives played a major part in Hull's theory, the inclusion of all of them served to weaken considerably the presumptions of anti-nativism and environmentalism. His activity drive, in particular, foreshadowed the curiosity and exploratory drives which, because of their implications for behavioural autonomy, later became a major embarrassment to drive-reduction varieties of behaviourism.

Elementarism, environmentalism, 'mechanicism'²⁰, and the rest: the significance of these features was not just that they were included in Watson's position, but that they continued to be implicated in the popular (and even the professional) stereotype of behaviourism. The significance of their modification or tacit dismissal by later behaviourists was not just that it happened, but a) that it happened relatively easily, with no 'agonizing reappraisals' concerning their validity, and b) that different behaviourist psychologists chose altogether different theoretical features to retain, change, drop, or newly invent. Specific content was no more a defining characteristic of behaviourism's theories than it was of behaviourism's foundation.

Corresponding to the two sources of cohesiveness in behaviourist theories, the defining characteristics of behaviourism described

by Palermo (1971; see pp. 38-40 above) can be grouped into three classes--those determined by the rejection of the mental and other hypothetical internal factors; those derived from the continuity of behaviourist research with pre-behaviourist animal psychology; and those not in fact accepted by all of the major behaviourist theorists. Continuity with the earlier tradition in which behaviourism had its beginnings was responsible for the original emphasis on problems of learning and for the use of animal subjects. The demand for objectivity justified the pro tem concentration on relatively simple behaviour and the general distaste for emergent and nativistic factors in behaviourist theories (it is significant that these latter two were somewhat qualified by Hull and Spence). Finally, adherence to S-R formulations was rejected, and the 'passive organism' model was severely weakened, by Tolman and by those who followed his lead.

Nevertheless, most of the major figures associated with behaviourism--Guthrie, Hull, Krechevsky, Lashley, Miller, Skinner, Spence, etc.--continued to regard themselves as behaviourists, and felt that there was a definite continuity in behaviourist research. The question that arises is, are the divergent theoretical positions which they established sufficiently similar that they can profitably be viewed as various programmes in behaviourist normal science? But although this question is an obvious one, it cannot be given an unequivocal answer. It is to some extent an ambiguous matter, just how much cohesiveness is necessary for a scientific tradition to be considered as normal science. This is particularly the case when, as implied by the question just asked, 'cohesiveness' is assessed in terms of the similarity of theoretical positions and presuppositions, independent of the use to which these positions were put. Behaviour-

ist psychologists in general shared a common conception of objectivity, an emphasis on problems of learning, and a willingness to investigate these problems with animal and human subjects indifferently. They did not share any agreement on what is learned, how it is learned, or what class of theoretical conceptions (e.g., teleological, mechanistic) were most appropriate to account for the learning. Should greater emphasis be placed on what united behaviourists or on what divided them?

For some purposes, to do with determining just what it was that made behaviourism a recognizable orientation, it is obviously proper to emphasize what united them. But for others, and particularly for assessing the degree to which behaviourist research can be described as normal science, it is proper to emphasize what divided them, for the following reasons. Normal science, as Kuhn describes it, is such that it can be almost entirely cumulative. It consists in building up the body of science by accretion, by adding more and more bits to what is regarded as the common store of knowledge. While different normal scientists may disagree on particular theoretical matters, they agree on what they regard as fundamental and background matters. The issues on which they agree comprise the bulk of what they see as their science, and are always their starting point in conducting further research.

By contrast, behaviourism never included an agreed upon body of background knowledge in this sense. The various behaviourist theories each commenced with definitions of the subject matter of psychology, with statements of the classes of problems to be considered, and with an indication of how these problems were going to be approached. The whole scientific matrix was built from the ground up

each time. As a result, the theories advanced by different behaviourist psychologists were not merely different, they were also--for their proponents, who comprise the context in which it matters--fundamentally different.

While behaviourist theories of learning had some common ground, as described above, it was not their similarities that served as the basis for experimentation and theoretical development so much as their differences. The answers to questions such as: What is learned? (responses, S-R associations, expectations, relationships); How is it learned? (reinforcement, contiguity, confirmation); What if anything is reinforcement? (drive reduction, tissue need satisfaction, change in the stimulus array, a particular kind of stimulus, a particular kind of response)--these and a number of other contentious issues defined the principal differences between the major behaviourist theories. They also determined the course and direction of most behaviourist research. Each theory had answers to these questions, and a major part of the theoretical and experimental work performed by the proponents of each theory constituted an attempt to show that their theory's answers were right and that a competing theory's answers were wrong. The attempt to do in a rival, or to avoid being done in, was often the principal factor behind the introduction of new apparatus (e.g., the Lashley jumping stand), the demonstration of new experimental phenomena (e.g., sensory preconditioning, the reward values of saccharin), and modifications to existing theory (e.g., Hull's r_G , Tolman's 'motor pattern' learning). None of this is characteristic of normal science.

It is certainly true, nevertheless, that many behaviourists performed minute and intricate experiments specifically in order to

fill in the details in an accepted theoretical schema, and hence acted in a way unquestionably typical of normal science. The way in which they did this, however, reveals differences from paradigm-based normal science that are as instructive as are the similarities. The context in which any behaviourist psychologists were able to work in this way, and the background which they could take as established, were specific to the particular theoretical tradition within behaviourism (e.g., Hullian or Tolmanian) with which they were affiliated; they were not ones shared by all or even by most behaviourists. There was something like normal science going on in behaviourist psychology, but it was Hullian normal science, Tolmanian normal science, Guthrian and Skinnerian normal science--all of them different--and not simply behaviourist normal science. Similarly, there was something like a paradigm directing research in each of these mini-traditions, but it was a Hullian paradigm, a Tolmanian paradigm, etc., and not simply a behaviourist paradigm.

The difference is an important one. Since each group of behaviourist practitioners--whom we may loosely designate as a school--had to build up their science from its theoretical foundations, and spent much of their scientific energy in inter-school rivalry, the practice of behaviourist psychology never became genuinely cumulative. No theory was able to progress with any degree of scientific certainty very far beyond its particular laboratory base. Each was prevented from doing so, first by the existence of the others as serious rivals and hence as foci of attention; and also by the consideration that made the multiplicity of schools possible, that is, the absence of a major scientific achievement that they could or would jointly accept as providing the basis on which to build their theories. Instead,

each theory, consistent with Kuhn's description of a pre-paradigm school, handled a certain class of problems particularly well. Tolman's theory was most suited to accounting for the determinants of behaviour at a choice point; Hull's to relating response strength to drive level and to the interrelationships between drives in a single-response experimental situation; Guthrie's to accounting for changes in behaviour as a function of arbitrary changes in the stimulus array; Skinner's to controlling the detailed topography of isolated responses of any desired degree of complexity.

Extensions of the theories were often directed towards accommodating some of the phenomena emphasized by a competing theory; Palermo's example, the extension of Hullian theory to cover transposition behaviour, was of this sort. Such kinds of extension were appropriate to the competitive scientific climate of the times, but made problem selection a relatively ad hoc procedure. More general extensions of theory to complex human behaviour--the ostensible goal of most behaviourist theories--were typically not genuine extensions of theory at all, but schematic applications, often resting heavily on analogy, of existing theoretical concepts to the description of social situations. As Koch points out (Koch, 1964; cf. p. 20, above), such 'extensions', far from being stimulated by an appreciation that the experimentally based theories were sufficiently far advanced to deal with extra-laboratory behaviour, were rather prodded by the realization that such theories had in their laboratory context made no significant progress in approaching such real situations. As a result, the later theoretical extensions arising from more fully matured behaviourist theories (e.g., Dollard & Miller, 1950; Skinner, 1953, 1971) have been as programmatic and unspecific as were the recognizably premature but

optimistic earlier ones (e.g., Dollard et al., 1939).

In short, the plurality of schools, and the resulting necessity for each to address much of its research to the others, defined a situation in which long range, unidirectional, cumulative development was impossible. Behaviourism as a whole never possessed the unanimity of outlook necessary for the practice of normal science, and the individual schools within behaviourism were never sufficiently free of serious external challenges to devote themselves without distractions to articulation of their various theoretical positions. Their attempts to generalize their positions outside of their laboratory context were, in the absence of a developed scientific tradition sufficiently viable to provide detailed empirical foundations for them, fated always to be premature.

III. Conclusion.

The exposition in this chapter has proceeded largely by contrasting Palermo's view of behaviourism with mine, in order to highlight some features of both views. This procedure is convenient for expository purposes, and it is also appropriate, since the differences between our views are in certain respects very important. However, placing emphasis on our differences may serve to conceal the extent of our agreement, and it is necessary to put both into perspective.

Palermo and I, along with many other psychologists, would agree on all the following statements. Behaviourism has been a dominant but diffuse movement in American experimental psychology for at least the past fifty years. During approximately the past twenty of those years it has been confronted with a growing number of unresolved conceptual, methodological, and empirical difficulties. Attempts to meet these difficulties have not been altogether successful, and have

resulted mainly in the movement's growing even more diffuse. These unresolved difficulties have been instrumental in leading to a rejection of behaviourism on a scale which by now could be described as revolutionary.

What divides us is the interpretation of these events, based on our different assessments of the foundations of behaviourism. I maintain that while experimental psychology may be said to be undergoing a revolution, it is not a Kuhnian revolution. The conclusion following from the analysis presented so far is that a Kuhnian interpretation cannot profitably be applied to the career of behaviourism; behaviourism was not based on a paradigm, and consequently it did not have the means to settle what were taken to be fundamental substantive issues, in the way necessary for the practice of paradigm-based normal science. This difference in interpretation is, however, only of narrowly academic interest unless it leads to different predictions, interpretations, or recommendations for post-behaviourist psychology. To employ the pragmatic criterion, it is a difference that makes a difference only if it has implications that go beyond psychology's past history.

And, as we shall see, the difference does make a difference in precisely that way. Palermo makes no critical mention of what I take to be the fundamental methodological error of behaviourism, its methodological objectivism as such. Methodological objectivism and Kuhnian paradigms are incompatible, not only as interpretive schemas for the rational reconstruction of science, but also as different bases for practice within a science.

A brief clarification is in order, both of how these two are incompatible and of what it means to say that methodological objectivism is an error. Mention was made previously of what could loosely be

called competing schools within behaviourism. This plurality of schools did not arise sui generis. It was closely linked, as indicated, to the absence of a paradigm that could function effectively for all of behaviourism. To generalize the point beyond a specific concern with Kuhnian paradigms as such--as will be done throughout the rest of this essay--it was linked to the absence of any kind of defining contentual base for behaviourism. It is of crucial importance, furthermore, that this absence of a defining content in behaviourism was quite intentional. There were, as we have seen, scientific achievements that might have provided the basis for behaviourist research in such a way that that research would have exhibited the Kuhnian pattern. Either Thorndike's researches or Pavlov's, or a combination of them, were likely candidates for a paradigm in this sense.

However, it was no part of--it was incompatible with--the objectivist and aggressively scientific programme of behaviourism to go any farther than rigorous logic required in accepting a shared commitment either to a concrete achievement or to a specific set of systematic hypotheses. Any shared commitment of that sort would violate what behaviourists did share, which was their conception of science. What united behaviourists was their conviction that the methodology of science, rather than its content, was what constituted an activity specifically as scientific; and that methodological considerations provided a sufficient basis on which to build scientific systems. Commitment to a theory or to a point of view was, at the very most an individual matter. Commitment to the procedures of science was the main shared characteristic of the group. The behaviourists came to agree, that is, that a set of decision procedures

for evaluating research, appropriate to all sciences indifferently, was the principal requirement for the constitution of a science; that with these decision procedures determined the content of scientific theories would be self-correcting; and that once their science possessed these it would as a result acquire continually increasing systematic validity as it continued to develop; and in all this they were wrong. Why they were wrong, that is, why their conception of science was such that its implementation could not fulfill their aims, is the major part of the subject of the following chapters.

Chapter 3

Positivism, Realism, and Behaviourist Psychology

As we have seen, behaviourism consisted largely in an attempt to base psychology on the practices of the physical sciences. As such, there is nothing very new in this attempt; for centuries, it has been quite typical for psychologists and mental philosophers to try in one way or another to base their discipline on the practices of physics and related sciences. The trend towards not merely relating physics and psychology but explicitly basing the latter on the former has been a strong, even the dominant one, since the beginning of modern science in the seventeenth century. Hobbes (1655) and Gassendi (1658), for instance, began implementing the trend in its modern and systematic form by making thorough analyses of perception, cognition, and memory as purely material processes, thereby translating the mind into a physical system operating according to the principles of Galilean dynamics. Their theories were thus marked by a simple equation of mental and physical, or assimilation of the former to the latter, of a sort which, because of the sharp epistemological distinction between knower and object of knowledge required by the new sciences, appeared unnecessarily crude¹. This crude materialism of their theories was, accordingly, decisively eliminated by Locke, who declined to "meddle with the physical consideration of mind; or trouble myself to examine wherein its essence consists; and whether...ideas do in their formation, any or all of them, depend on matter or not (Locke, 1690, I, 1, 2; Fraser ed., p. 26)," and substituted for an avowed materialism an autonomously mental atomism, explicitly analogous to Newton's physical atomism. It is with Locke, in fact, that mental elements, separate

from but having properties derived from those of physical atoms or particles, may be said to have become influential in modern (i.e., post-Renaissance) psychology. To cite a somewhat later example, Hume declared on the title page of his Treatise of Human Nature (1738) that it was "an attempt to introduce the experimental method of reasoning into moral subjects." The description indicates the ideal he was following even although, as a later commentator observed, "In following this line Hume reveals how little he realized what is meant by experiment (Drever, 1968, p. 14)." Klein (1970), in his history of psychology, cites further examples; the quotation is taken from a commemorative article on Alexander Bain.

It may be affirmed generally that the advance in psychology in our land has very much followed the advance in physical research. The theory of sound, for instance, was the outstanding physical theory in the time of Hartley. Consequently, he proceeded to interpret mind according to the analogy, and represent the nervous processes as simply propagations of vibrations as in sound. Chemistry, in like manner, came to the front in the days of [J. S.] Mill. Consequently the process of Association was interpreted in terms thereof--it was set forth as a kind of mental chemistry. So, in Dr. Bain's time, physiology was attracting much attention, and the work of Johannes Müller, in particular, was greatly in evidence, and there was also an awakened interest in biology. Hence the physiological reference became prominent, and the method of natural history pointed the way to Dr. Bain's mode of procedure (Davidson, 1904; quoted in Klein, 1970, p. 803).

And Klein adds later:

Just as the chemists of Wundt's era regarded chemical elements as irreducible units of chemical analysis, so Wundt regarded sensations as irreducible units of psychological analysis. As elements, they could not be analyzed into still simpler units, but they could interact with one another to form chemical and psychological compounds, respectively (Klein, 1970, p. 853).

Still further examples could be mentioned, to show how even the fine structure of psychological and physical theories have been related². The point which they jointly establish is that the content

of psychological theories, particularly in traditions continuous with that of British empiricism, has in large measure consisted in the analogical application of principles and concepts derived from currently popular physical theories.

This kind of attempt to base psychology on physics by making an analogical application of physical theories is not, however, of principal interest here. It is certainly true that behaviourist psychology has not been free of this practice of borrowing its concepts and models from the physical sciences. As mentioned in the last chapter (p. 67), Weiss (1925) attempted a reduction, equally logical and theoretical, of psychology to atomic physics. The outstanding contemporary example of borrowed concepts, popular for the past twenty years or more, is that of cybernetics and information-processing models of perception and cognition. The practice of borrowing models has never been applied systematically within behaviourism, however, and has never been a central characteristic of the movement. Instead, what was explicitly taken as the basis of behaviourism, and what thus marked a new or relatively new turn in psychology's scientific history, was a strict commitment to scientific method, with relatively little prior commitment to particular types of scientific theories. This emphasis on scientific method shows clearly in the examples given in the preceding chapter--Watson's emphasis on observation and verification, logic and mathematics ("the tools of every scientist"), Skinner's early espousal of operationism, Hull's seeking a methodological paradigm in Galileo's astronomical theories, etc.

An emphasis on scientific method, as abstracted from the practices and methodological analyses of the physical sciences was, if not altogether new in psychology, at least new in the extent of its

systematic application and in its clear demarcation from concern with physical theories. The adoption of both the observational procedures and the logic of physics, without any corresponding systematic adoption of the theories of physics, was unprecedented. Wundt, and following him Titchener, attempted in their theory construction to mirror the logical procedures of the physical sciences--and, as suggested in the quotation from Klein, above, also adopted a conceptual scheme based on that of chemistry--but they made a sharp distinction between the observational procedures appropriate to such sciences and those appropriate to psychology. Hume, perhaps, may be said to have attempted to implement in his Treatise a methodological programme similar to that which we are considering here, but he failed actually to implement either the observational or the logical aspect of experimental scientific inquiry as it was practiced at the time.

Scientific method, however, is itself a concept or procedure that is hardly free from ambiguity. The systematic results of physical scientific enquiry have made those 'senior' sciences objects of emulation for centuries--much as geometry was in Francis Bacon's time--but the systematic methods for obtaining those results have been the subject of nearly as many divergent formulations as there have been theorists to formulate them. The problems of explicating and, where appropriate, formalizing these methods in their most general form (apart, that is, from specific questions of instrumentation, etc.) have comprised nearly the entire subject matter of the philosophy of science, considered as a discipline, and the philosophy of science has long been as heterogeneous a discipline as one could hope to find. Whether the route to knowledge is held to pass through induction, deduction, or intuition; falsification, hypothetico-deduction, or

empathic projection; abstraction, picturization, or concretization, there have been theoreticians to argue convincingly the merits of their favoured route and, frequently, to relate it to a general epistemological theory (a neat summary of the historical panoply of methodological views advanced from Aristotle to Popper is given by Losee, 1972).

Even to say, therefore, that behaviourism consisted largely in the adoption of a rigorous scientific method is insufficient to characterize the movement, for the conception of scientific method adopted within behaviourism was only one of many possible ones. It is not hard, nonetheless, to delimit the kind of conception which was implemented. That conception was of course hard-headedly empiricist, and correspondingly anti-rationalist and anti-intuitionist. It was indifferently inductivist and deductivist, in that it encompassed both procedures without strain. All of this, behaviourism shared with various forms of introspective psychology. What differentiated it was, again, the rejection of unobservables, a rejection explicitly implemented as a methodological maxim. Behaviourism, in short, adopted--and in large measure defined itself by adopting--a conception of scientific method that involved a strong commitment to observations and logical analyses, a rejection of any concern with unobservables, and a corresponding unwillingness to extrapolate beyond observables in the systematic interpretation of data. In Chapter 4 it will be shown how this conception was initially developed, in a piecemeal fashion, in behaviourist and immediately pre-behaviourist psychology. Considered as a systematic conception, as it eventually came to be considered within behaviourism, it was one which had close affinities with the methodological programme advanced by Karl Pearson in his

The Grammar of Science (1895) and also, with some reservations, with that defended by Ernst Mach in his The Science of Mechanics (1883)--a conception of science, that is, that can generally be described as positivistic.

The implication of positivism enables the discussion to become more specific. To speak of behaviourism as being founded on a positivistic methodological orientation toward the practice of psychology is of course to make a descriptive generalization about the movement, just as it was a descriptive generalization earlier to describe the movement as being founded on a position of methodological objectivism. The present one is, furthermore, a generalization that would have received willing assent from many prominent behaviourists during the movement's heyday (cf. Stevens, 1939). But in this case, more is involved than simple description; it is a description with explanatory power. It is a theme of central importance in this monograph that the label 'positivism' denotes a recognizable natural family of approaches to science, and that much of the career of behaviourism, its successes and failures alike, is interpretable specifically through an analysis of the implications and consequences of adopting such an approach within psychology. That is, the career of behaviourism is intimately bound up with the potentialities and limitations of a positivistic orientation toward science. It therefore follows, at least on the account which will be presented here, that it is impossible fully to understand the former without having some comparable understanding of the latter.

In modern times, both in science and in the philosophy of science, 'positivism' or 'logical positivism' usually refer to such things as the verifiability criterion and other techniques, or decision

procedures, which have been developed and used for the rigorous evaluation and testing of scientific statements, hypotheses, and theories. Indeed, it is not too much to say that the systematic use of such procedures sufficiently identifies and characterizes the implementation of a positivist orientation in modern science. Adherence to such decision procedures was, furthermore, characteristic of much of behaviourism, and was explicitly invoked as ensuring the scientific cast of mature behaviourist theorizing. A major part of the examination of positivism will, correspondingly, involve the analysis of the viability and effectiveness of these procedures for the purposes for which they were developed. Adherence to such explicit decision procedures is not, however, all there is to positivism. Such adherence merely constitutes the way in which a positivist orientation has most recently been implemented, in behaviourism as elsewhere, just as it was previously implemented mainly through the repudiation of unobservables as such--again, in behaviourism as elsewhere (e.g., by Mach and Pearson). But however a positivist orientation is implemented in science, it has certain general functions which it usually performs. Analysis of the role of positivism within behaviourism thus requires a prior consideration of what these general functions are. It is necessary first of all, therefore, to consider the general characteristics of positivism as such, particularly as it relates to the practice of science, and to illustrate the kinds of problems which arise in the course of such practice for which the rigorous implementation of a positivist orientation can be advanced as a legitimate solution.

Gaining this understanding of the characteristics and potentialities of positivism in the conduct of scientific enquiry will

require a rather extended excursion into matters which are usually considered to form part of the philosophy of science. It is an excursion, therefore, which will at times seem to have only the most tenuous contact with psychology as we know it. It cannot legitimately be avoided, nevertheless, primarily for the reason already given, that behaviourism cannot be understood apart from an analysis of the role that positivism played in it. More generally, in addressing scientific questions or those pertaining to the history of science, we can ignore what is considered the philosophy of science only so long as we have a consensus about those questions which the philosophy of science tries to answer--questions about how, in the most general terms, the science is to be practiced. Such a consensus existed for some time in behaviourist psychology, as we saw in Chapter 2, and was closely related to developments in positivist philosophy. As we saw in Chapter 1 however, that consensus is now in the midst of crumbling, both in psychology and in philosophy; but in psychology at least it has not yet shown any signs of being replaced by another. Analysis of the characteristics of the previous consensus may be a relevant procedure on the way to developing a successor.

The remainder of the present chapter will therefore discuss the general character and significance of positivism in science, considering its potentialities and limitations in as informal a manner as possible. Later chapters, on the basis of the description to be developed here, will consider how positivism came to be associated with behaviourism in the first place, and will present a somewhat more technical review of the decision procedures characteristic of modern logical positivism and of neobehaviourism alike.

First, however, it will be well to gain some perspective by

stating in a nutshell the conclusion to which the account will eventually lead. It is that the positivistic conception of science implemented within behaviourist psychology is generally appropriate at certain stages in the development of specific sciences--appropriate, that is, in the sense of facilitating the systematic development of the science--but that it is not appropriate at other stages; and particularly, that it was not fully appropriate throughout psychology at the time when behaviourism gained prominence. It will be argued that positivism is most capable of serving as an appropriate basis for ongoing scientific enquiry when it arises as a result of internal developments within a scientific discipline, and that it usually tends to do so when the experimental findings and conceptual analyses advanced within the discipline have carried that discipline into a state of turmoil of much the sort that Kuhn (1962) describes as a pre-revolutionary crisis state. However, positivism did not become involved in behaviourism in quite this way, but arose largely, and was maintained almost entirely, through an explicit copying of the autonomously positivistic orientation that was seen as dominant in the natural sciences at the time.

I. Positivism and Realism

as Contrasting Orientations toward Science.

By 'positivism' is not meant anything radically different from that term's customary usage. Such usage, however, is often vague and ambiguous, and the first task in relating positivism to behaviourism is therefore that of making a systematic characterization of positivism.

As a beginning, positivism is certainly a tough-minded attitude towards science and philosophy. It rejects all or most metaphysics

as scientifically sterile and advocates the restriction of attention to publically observable data. Beyond this basic characterization, however, there is room for question as to just what positivism is and where it comes from. In one sense, positivism originated with Auguste Comte, who coined the term and made it the basis for his mildly scientific social philosophy. In another sense, quite opposed to Comte's, positivism is a very general and very ancient way, common since the time of Aristotle or, at the latest, that of Ptolemy, of being cautious about claims of scientific validity or, in general, statements of supposed fact. In yet a third sense, most familiar today, positivism is a kind of technical philosophical analysis for distinguishing between meaningful and nonsensical statements. It is therefore at least slightly arbitrary just what we choose to call 'positivism'. Nevertheless, the last two descriptions, and perhaps even all three, have enough points in common that a useful general characterization might be attempted.

A good starting point for such a general characterization is the treatment of positivism in Kolakowski's thoughtful little book, Positivist Philosophy from Hume to the Vienna Circle (Kolakowski, 1972). Kolakowski outlines four maxims or rules which he suggests are generally typical of positivist thought. The first, and the basis for all the rest, is the rule of phenomenalism, which states that "there is no real difference between 'essence' and 'phenomenon' (Kolakowski, 1972, p. 11)." Anything that cannot be manifest in a purely phenomenal way, such as the Kantian ding an sich or the scholastic essences, has no place in scientific thought.

According to positivism, the distinction between essences and phenomena should be eliminated from science on the ground that it is misleading. We are entitled to record only that which is actually manifested in experience;

opinions concerning occult entities of which experienced things are supposedly the manifestations are untrustworthy. Disagreements over questions that go beyond the domain of experience are purely verbal in character (ibid., pp. 11-12).

The second is the rule of nominalism, which states that "we may not assume that any insight formulated in general terms can have any real referents other than individual concrete objects (ibid., p. 13)." Since it is only such "individual concrete objects" that can ever be phenomenally apparent, this rule obviously follows from the preceding one. However, the emphasis is somewhat different, and in addition, the intensity of the venerable debate over universals and particulars--which is historically somewhat separate from that over phenomena and essences--makes it advisable that this rule be formulated separately. There is no such thing--or we should not assume that there is any such thing--as mankind over and above all individual persons; the inverse square law does not apply (or should not be considered to apply) except between specific, particular bodies, etc. It is true that while perfect circles, perfectly uniform acceleration, etc., are never encountered in experience, we are nevertheless justified in assuming them as limiting or ideal cases in our equations and theories. But we should not assume on that account that they exist or occur somewhere in reality.

A system ordering our experiences must be such that it does not introduce into experience more entities than are obtained in experience. But since it inevitably uses abstractions among its means it must also be such that we do not forget that these abstractions are no more or less than means, human creations that serve to organize experience but that are not entitled to lay claim to any separate existence (ibid., p. 15).

The third rule of positivism is the rule that refuses to call value judgments and normative statements knowledge, or in short, a rule expressing the fact-value distinction.

Experience, positivism argues, contains no such qualities of men, events, or things, as 'noble', 'ignoble', 'good', 'evil', 'beautiful', 'ugly', etc. Nor can any experience oblige us, through any logical operations whatever, to accept statements containing commandments or prohibitions, telling us to do something or not to do it (ibid., p. 16).

This rule can also be shown to follow from the rule of phenomenalism. Moral and aesthetic qualities may be considered to be exemplified by a thing or event--that is, a thing or event may be taken as an exemplar for the attribution of a moral or aesthetic quality--but such qualities are not phenomenally manifest in the thing or event as anything distinguishable from its other, unambiguously phenomenal qualities.

For on the phenomenalist rule we are obliged to reject the assumption of values as characteristics of the world for they are not discoverable in the same way as the only kind of knowledge worthy of the name. At the same time the rule of nominalism obliges us to reject the assumption that beyond the visible world there exists a domain of values 'in themselves', with which our evaluations are correlated in some mysterious way. Consequently, we are entitled to express value judgments on the human world, but we are not entitled to assume that our grounds for making them are scientific; in other words, the only grounds for making them are our own arbitrary choices (ibid., p. 17).

In effect, this rule constitutes a warning against committing the naturalist fallacy, that is, against assuming that goodness and beauty are properties of things or events rather than simply judgments about them or, more unequivocally, reactions to them. Moral and aesthetic questions are clearly "questions that go beyond the domain of experience" and are hence "purely verbal in character"--or at most, psychological.

The fourth rule, somewhat tenuously connected with the rest, is a belief in the essential unity of the scientific method.

In its most general form it expresses the belief that the methods for acquiring valid knowledge, and the main stages for elaborating experience through theoretical

reflection, are essentially the same in all spheres of experience. Consequently we have no reason to assume that the qualitative differences between particular sciences come to anything more than characteristics of a particular historical stage in the development of science; we may expect that further progress will gradually eliminate such differences or even, as many authors have believed, will reduce all the domains of knowledge to a single science (*ibid.*, pp. 17-18).

This belief in the eventual unity of science, and particularly the faith or assumption that the qualitative differences between sciences can be accounted for in terms of their stages of historical development, is the main link between Comtean positivism and the other two types mentioned. However, this belief is a central guiding assumption for Comtean positivism, while for the others it is at best tentative and programmatic, arising purely as a consequence of the demarcation of science as that body of systematic inquiry that adheres to the other three rules. In particular, for modern logical positivism, the unity of science is based on almost purely logical considerations, with historical analysis given at best peripheral status.

Phenomenalism, nominalism, the fact-value distinction, and belief in the eventual unity of science: how far do these go towards providing a general account of positivism? It is certainly true that identifiably positivist thought often reflects these features, and that consequently they convey much of the flavour of positivism; and Kolakowski was initially trying to do no more than provide such a summary characterization. But to consider these features as jointly essential to positivism, or as constitutive of it, would lead to grave difficulties. In defending these features as definitive of positivism later on, Kolakowski recognizes some of these difficulties and attempts to deal with them. Galileo, Descartes, and Leibniz, for instance,

...shared the positivist conviction that interpretation of the world in terms of unseen faculties or forces, in-

accessible to empirical investigation, is absurd... Though he clung to the concept of substance, Descartes tried to characterize it in such a way that it lost its old mysteriousness; matter, or extended substance, is nothing but extension, and the soul, or thinking substance, is nothing but thinking. There is no 'nature' hidden behind the actually observed qualities of things, reference to which accounts for anything whatsoever... Although Descartes did not carry this position to its ultimate consequences, and was not perfectly consistent in asserting it, it certainly is in line with the positivist programme (ibid., p. 34).

However, Descartes' position is similar to that of positivist thought only in the one, certainly important, respect that both positions involve an attempt to demystify metaphysical concepts. More important than this similarity is that the methods and the reasons for doing so have practically nothing in common in the two positions. Descartes, like Galileo and Leibniz, was a realist, a rationalist, and at least half a Platonist. All three attempted to intuit non-phenomenal reality of some sort, had little faith in the capabilities of empirical investigations unless they were guided by independent intuitions of the truth, and shared with identifiably positivist thought little more than a distaste for the obscurantism of the later scholastics. But if this distaste is expressed, as Kolakowski expresses it, as a denial of 'essences' which lie behind and support phenomena--if it is described, that is, without the qualification that the 'essences' thus adjoined were not identical with non-phenomenal reality in general but were instead the focal point of one particular sterile system of metaphysics--then it does indeed seem to mark these otherwise very un-positivistic thinkers as strangely sympathetic to the central phenomenalist tenet of positivism.

Kolakowski attempts to minimize the paradoxical consequences of considering Descartes as a positivist by suggesting that he was only

partly a positivist.

Thus, if mere denial of non-phenomenal 'essences' sufficed to earn a thinker the title 'positivist', Descartes (like Leibniz) would be a fully-fledged representative of the tradition. But because, at least in the light of the development of positivism in the last two centuries, this criterion can hardly be considered sufficient, Descartes can be called a positivist only with serious reservations (ibid., p. 35).

The reservations concern the distinction between analytic and synthetic statements, or between necessary and contingent truths, according to their epistemological source, their form of validity, and their informational content about the world. Descartes' positivist credentials are weakened because he did not observe the modern form of this distinction and attempted to apply necessary truths in characterizing the world as it actually exists. But this facet of Descartes' thought was of crucial epistemological significance to him. More than a reservation concerning the extent of his adherence to positivism, it constituted a complete repudiation of the phenomenalist assimilation of phenomena and essences, as well, of course, as a complete rejection of empiricism and of the necessity for empirical verification. Descartes, again, was 'anti-essentialist' only if the concept of 'essence' is restricted to its technical use in scholastic philosophy. Insofar as essences refer generally to the reality that is separate from and supportive of phenomena, he was clearly an essentialist. Furthermore, it cannot be maintained that Descartes' failure (if it was a failure) to apply the analytic-synthetic distinction in his systematic theorizing was due to the unavailability of the distinction prior to another two centuries of development of positivist thought; the distinction between factual truth and logical validity was a commonplace one in the medieval logic in which Descartes was trained. (Even less, incidentally, can such an excuse be maintained for Leibniz, who was instru-

mental in extending the distinction and making it fundamental to modern logic). Rather, the analytic-synthetic distinction is not one which can profitably be applied to Descartes' philosophy at all. His a priori and necessary truths were not founded in logic, that is, in application of syllogistic, so much as they were based on considerations of ontology, that is, on the direct inspection and analysis of the implications and characteristics of being.

Thus, Descartes' "denial of non-phenomenal essences" did not constitute a denial of non-phenomenal reality or of its availability to scientific investigation, and it is consequently difficult to credit him as a positivist. A precisely opposite difficulty in the characterization of positivism can be found in the early twentieth century movement in the philosophy of science known as conventionalism, whose adherents often did believe in non-phenomenal essences--and often scholastic ones at that--but were positivists nonetheless. Conventionalism is clearly in the historical mainstream of positivist thought. It was in part an outgrowth and extension of Mach's positivism, and had a major influence on the development of logical positivism (Frank, 1949a). However, the leading exponents of conventionalism--Poincaré, Duhem, and LeRoy in France, Dingler in Germany--were neither phenomenologists nor nominalists. Poincaré and Duhem were both conservative Catholics, intellectually Thomist but theologically more inclined to Augustinianism, emphasizing the necessity for grace and otherworldly faith equally for salvation and for the achievement of any insight into the Absolute. LeRoy was a popularizer of Bergson's theories of intuition and direct contemplation of reality. Dingler was a philosopher in the German voluntaristic tradition of Fichte and Schopenhauer, according to which our conception of nature (some would say nature

itself) is an untrammelled creative act of the will. These philosophers were all transcendentalist in their conception of reality; what made them positivists was, as will become clear, their analyses of the methodology and epistemology of science. Their non-scientific convictions concerning the nature of reality were defined as precisely that--non-scientific; they were sharply distinguished from, and were related only negatively (albeit closely) to their theorizing about science.

In short, the four central characteristics adduced by Kola-kowski are insufficient as a general characterization of positivism, because some positivist thought does not incorporate them and some non-positivist thought does. It is necessary to consider the relationship between science and the real world, in the conceptions of the thinkers mentioned, to clarify what it is that makes Poincaré and Duhem positivists but Descartes and Leibniz something else.

The distinction between these two pairs of philosophers lies in the different status which they accorded to their scientific theories. Descartes and Leibniz believed that their theories were true or, at worst, false. That is, for them the validity and worth of their theories consisted in their accord with reality; since reality was partly phenomenal and partly non-phenomenal, any valid theory had likewise to incorporate phenomenal and non-phenomenal references. Poincaré and Duhem, on the other hand, maintained that their theories--and all scientific theories--were neither true nor false, but merely more or less useful. For them, the validity and worth of their theories consisted solely in their accord with observational predictions deduced from them. The corpuscles and monads with which Descartes and Leibniz filled the universe were, although strange and incomprehensible to us,

assumed to be real. The space, time, mass, and gravitation with which Poincaré and Duhem filled the universe were, although more familiar and customary to us, assumed to be irreducible hypothetical. Since these entities, forces, etc., are merely hypothetical, they can whenever they outlive their usefulness be replaced with other hypotheses, without such replacement necessitating any basic changes in our comprehension or apprehension of reality. Regardless of the nature of reality--and Poincaré and Duhem were in complete agreement with Descartes and Leibniz that much of reality is not phenomenal--the scope of applicability of science is perforce restricted to the phenomenal realm. Science, for the conventionalists, does not give access to a trans-empirical or trans-phenomenal realm of essences, things in themselves, grace, or reality--and this property marks a genuine limitation of science. This trans-phenomenal realm was of great importance to the conventionalists, as it was historically for many other positivists, but it was not the realm of science. Science deals only with the observable--and for the conventionalists even that was not absolute and fixed, but was in large part a product of previous experience, expectations, 'knowledge', etc.--and as a result science delivers only the useful or workable.

This distinction between conventionalists such as Poincaré and Duhem and rationalists such as Descartes and Leibniz can be extended to cover positivists and non-positivists--to whom the term 'realists' will be seen to be applicable--in general. Mach's positivism was not the handmaiden to theology that Duhem's sometimes seemed to be. Mach had no interest in metaphysics of any sort, and was concerned only to establish the purely empirical character of science. To this end he made searching criticisms of scientific concepts which

were assumed to have universal applicability, such as space and time in Newtonian mechanics. Mach maintained that there was no way in which such universal concepts could have empirical significance, that they should therefore be considered inadmissible in any empirically oriented science, and that the unwarranted extension of the concepts of space and time to universal status was largely responsible for the (then) current crisis in physical explanation. All of these scientific concepts were founded in our perceptions, Mach insisted, and as a result they could not validly be extended beyond the reach of perceptions. For Mach, the function of scientific theories--their only possible function--is to provide the most economical arrangement and classification of our perceptual observations³.

In opposition to Mach we may place Planck--and this opposition was a real one at the time. If Mach's positivism was not in the service of the transcendent, neither was Planck's rejection of it. Unlike Descartes and Leibniz, Planck made no systematic claims in favour of a rationalistic source of knowledge or justification of knowledge claims, or of a reality that had any transcendent characteristics of any sort. With those philosophers however, and even more forcefully, Planck insisted that the goal of science was to uncover the truth about nature, and he resisted all hedgings concerning the domain of applicability of the truth that was thus sought. Not only, he maintained, was science a search after truth, but science was incapable of development, was even inconceivable, in the absence of this search. In particular, he stressed that any such goal as Mach's principle of economy of description of observed phenomena was inadequate as a basis on which to account for scientific activity as it

was actually practiced. In a paper published in 1909 Planck stated:

When the great masters of exact investigation of nature gave their ideas to science, when Nicholas Copernicus removed the earth from the center of the universe, when Johannes Kepler formulated the laws named after him, when Isaac Newton discovered gravitation...--the series could be long continued--surely, economical points of view were the very last thing to steel these men in their struggle against traditional opinions and dominating authorities. No, it was their unshakeable belief--whether resting on an artistic, or on a religious basis, --in the reality of their world picture. In view of these certainly incontestable facts, one cannot reject the surmise that, if the Mach principle of economy were really to be put at the center of the theory of knowledge, the trains of thought of such leading spirits would be disturbed, the flight of their imagination crippled, and consequently the progress of science perhaps fatefully hindered (quoted in Frank, 1949a, p. 63).

The distinction between Descartes, Leibniz, and Planck on the one hand, and Poincaré, Duhem, and Mach on the other, can best be summed up as a distinction between positivism and realism, and this opposition between the two orientations towards science serves to characterize them. The most general characteristic of positivism, on this view, is a refusal to ascribe realistic significance to a scientific theory. More precisely, it is a systematic suspension of judgment, or a denial of the possibility or meaningfulness of judgment, concerning the absolute truth or falsity of a scientific theory. Scientific theories are neither true nor false--truth and falsity are judgments not applicable to them--but only useful or useless, economical or uneconomical. Hypothesized entities and forces are neither real (existent) nor unreal (nonexistent), but only and inescapably provisional. Thus, any judgments about whether a theory is true or false, or about whether it represents reality or not, are barred as a matter of principle from being relevant to the assessment of the theory. The truth or falsity of a theory is not available as a criterion for judging it, and the judgment that a theory is true thus adds nothing to its empirical content.

"There are almost incontestable logical reasons for this suspension or abjuration of judgment. Any such judgment goes beyond any observations which can provide empirical support for the theory, and to that extent is not even in principle confirmable or disconfirmable by reference to such observations. If reality consists in anything other than what we observe, then our observations cannot of themselves reveal it to us, or even reveal the fact that it so consists. Conversely, if reality does not consist in anything other than what we observe (a position we might call metaphysical phenomenalism or, in another form, Berkeleyian idealism), then our observations cannot reveal that to us either. As a result, the question of what reality consists in is a question which empirical data cannot be used to answer; any answer to the question is empirically meaningless. It follows that the question is not one that has any place in science.

Thus, the systematic suspension or abjuration of judgment can also be characterized as a repudiation of all empirically unverifiable or 'metaphysical' pronouncements or concepts in science. It constitutes, therefore, a general form of the verifiability criterion associated with logical positivism. The verifiability criterion asserts, in its simplest formulation, that the meaning of a statement is identical with or is given by the operations and observations that would constitute its verification. The intended consequence of this criterion is that, of all possible synthetic statements--statements purporting to give some factual information about the world--only those ones which can be empirically verified can be considered to have any genuine factual meaning. All others--but not, of course, including analytic statements--are categorized as meaningless and

metaphysical⁴. The verifiability criterion has, through its vigorous promulgation, been sufficiently influential that lack of empirical verifiability has come to be accepted almost as a defining characteristic of metaphysical statements. Its influence has not been sufficient, it is true, to secure general agreement that all such statements are meaningless. Still, the criterion has been accepted as sufficient at least to demonstrate that scientific theories can and should have nothing to do with metaphysics.

The field is thereby left open, however, for metaphysical--that is, non-empirical, non-scientific--theories to claim access to a higher kind of truth than that available to scientific theories. The access of such non-empirical theories to a higher truth cannot be strictly gainsaid on scientific grounds, so long as their proponents are careful not to incorporate any empirically meaningful statements into their theories. This restriction is one which some philosophers have found themselves able to meet, and the resulting opportunity for a complementary relationship between positivism and non-empirical or metaphysical philosophy has been a source of considerable ambiguity within positivist philosophy, one that at times has occasioned severe discomfort. Some positivist philosophers, such as Poincaré and Duhem, have promoted positivist analyses as a means for clear demarcation between science and metaphysics, a demarcation salutary to the progress of both. Others, such as Mach, have disdained any interest in metaphysics at all, and wished merely to get on with their science. Still others, and particularly the logical positivists of the Vienna Circle, were vigorously opposed to any sort of rapprochement between science and metaphysics; they took it as their aim to effect the complete banishment of metaphysics, or at least the demonstration that metaphysics

could have none other than poetic or emotional significance.

In all fairness, however, it must be said that this aim of the logical positivists was a failure from the outset. For on their own principles, there was no way that the verifiability criterion--the touchstone for distinguishing between meaningful and meaningless statements--could have any other than stipulative significance. This property of the verifiability criterion--that in other words it was itself empirically meaningless--was a formal embarrassment to logical positivism from the beginning, although not one that was worried about unduly. A statement that could not be empirically verified could be called meaningless only by restricting the domain of meaningful statements to those which were empirically verifiable, and this restriction of course could not itself be justified on empirical grounds. The verifiability criterion of meaning thus reduces immediately to, at best, a criterion of demarcation between empirical science and everything else, particularly between science and metaphysics. This purely demarcative function of the verifiability criterion was sufficiently obvious at the time it was first promulgated that, despite the hostility of logical positivism to metaphysics, many metaphysicians welcomed the movement with open arms. It seemed to them that logical positivism would safeguard the status of metaphysics as much as that of science. Phillip Frank, one of the earliest members of the Vienna Circle and a particular opponent of scholastic philosophy, bleakly instantiates this trend.

The French Catholic philosopher J. Maritain, at the Thomistic Congress in Rome in the summer of 1936, characterized as a great service that was essential also for Catholic philosophy the fact that the aim of the Vienna Circle and of the whole movement of logical empiricism was 'to disontologize science' (Frank, 1949a, p. 175).

Thus, from the point of view of the metaphysicians, logical positivism

provided a demonstration that scientists had no business encroaching on their territory, which included all questions concerning ultimate reality. Where empirical data are relevant they cannot, indeed, be transcended, but for the most important questions they are not always relevant and must then quite properly be ignored.

Realism, as the general alternative to positivism, has rarely received any systematic formulation as such, since it includes any theoretical or methodological scheme that is taken to lead to the truth about the world. In general, scientific realism is the conviction that scientific inquiry is capable of revealing the truth about the world, or about that part of it being studied. It carries also the accompanying conviction, as expressed in the quotation from Planck, that this truth cannot be assigned limits corresponding to the limitations of the experimental procedures employed in its determination. The truth that is sought by science always has a wider domain of applicability than that of the experiments utilized in the search; typically, the truths sought are universal ones. In relation to any specific scientific theory, it follows from a realist orientation that it is both meaningful and important to ask whether the theory is true or false. Coming to any conclusion regarding the truth or falsity of a scientific theory necessitates going beyond the available empirical evidence, and going beyond it in a direction in which, as was shown above, future empirical evidence cannot strictly follow. Thus, the maintaining of the truth or falsity of a scientific theory within a realist orientation necessarily involves affirming the theory to an extent greater than can ever possibly be strictly warrantable on the basis of empirical evidence and logic⁵.

Nevertheless, the truth that, on a realist account of

science, is revealed by scientific investigations, is not necessarily thought of as being wholly trans-phenomenal; but neither is it wholly phenomenal either, in the sense of being identified with phenomena. More characteristic would seem to be the belief, even if sometimes implicit, that while appearances can be deceiving, they can also, with proper selection and control, be revealing⁶. The unanalysed or uninterpreted phenomena of everyday life are insufficient to provide the truth about the world, because these phenomena are the final perceptible result of a long causal chain beginning with the complicated interacting forces which constitute those components or aspects of physical reality that are operative in one form or another in our immediate vicinity. These forces are themselves, however, at least in principle open to discovery, even in their most general form. They can be distinguished, separated, identified, and understood--but only through careful investigation, control, and measurement, or in short, through the sophisticated and careful practice of science.

The differing attitudes towards scientific theories and the resulting contrast between positivism and realism, as presented so far, can serve as the basis for specifying additional and subordinate characteristics of positivism. These are for the most part implicit in what has already been said, but can now be stated systematically and related to those described by Kolakowski.

The first subordinate characteristic of positivism is what may be called observationalism. This is simply the positive side of the repudiation of metaphysics, and implies a commitment to publically observable data, and to a correspondingly public data language, as the sole vehicle for making meaningful theoretical statements. Observa-

tionalism can be differentiated from phenomenalism, as Kolakowski describes it, in two ways. First, observationalism is neutral with respect to the choice between phenomenalistic and physicalistic descriptive languages, while phenomenalism at least seems to imply a commitment to phenomenalistic language. Different positivist formulations have in fact used both languages. Furthermore, as Frank (1949a) points out, the choice between them within positivist philosophy is a matter of convenience rather than one of principle; it is desirable to reflect this relative indifference in the characterization of positivism. Second, and more important, observationalism as described here has the character of a rule or convention more explicitly than does phenomenalism in Kolakowski's treatment. The difficulty with Kolakowski's description of both phenomenalism and nominalism is that he makes them seem perilously close to being metaphysical positions, and hence not really relevant to positivism at all. It is certainly not Kolakowski's intention to present phenomenalism and nominalism as metaphysical positions, that is, a theses about the constitution of reality; thus he speaks about the rules of phenomenalism and nominalism, thereby emphasizing their methodological character. Nevertheless, it was a consideration of the systematic and non-empirical content of phenomenalism and nominalism that provoked the difficulties over how to characterize Descartes and Leibniz. Positivism implies phenomenalism and nominalism, if it does at all, not as judgments about the real character of the world, and not as assumptions about the character of the world made for methodological or heuristic purposes, but only 'in effect'; that is, as a result of the refusal to go beyond observations or phenomena in search of 'real' entities. With these two minor reservations, observationalism and

and Kolakowski's rule of phenomenalism have the same import.

The significance of observationalism is that it directs attention to the procedures for acquiring knowledge or for validating knowledge claims at least as much as to the content of those knowledge claims. This characteristic of observationalism can be extended and generalized. For positivism in general, it is the methodology of scientific research--that is, the logical, observational, and experimental procedures appropriate to a field of inquiry--that constitute an activity as scientific. The content of scientific theories cannot exercise this function, nor can their form. Thus, the same string of words may comprise a metaphysical or a scientific statement, depending entirely on what operations are used to justify the statement. This emphasis on the methodology of science is not an all or nothing matter of course, rigidly separating positivism and realism. Some degree of emphasis on scientific method is typical of most scientific activity. But it is only as part of a positivist conception of science that the methods of inquiry can come to be of greater importance than the contents and systematic scope of theories for the assessment of a field as scientific. On a realist conception of science, the empirical and logical methods of science are justified instrumentally, by virtue of being the most powerful tools available for the study of nature. On a positivist conception, the empirical and logical methods are (if the phrase is permissible) of the essence of science, and science itself is justified only on broadly instrumental grounds.

The positivist emphasis on the methodological constitution of science is displayed to varying degrees in what can be identified as older positivist thought. In the early days of Copernican astronomy, for instance, the purely mathematical character of the astronomical

formulations was taken to provide a guarantee that the astronomical theory itself was purely scientific or 'mathematical' rather than metaphysical. The permissible function of the heliocentric hypothesis, like that of the detailed Ptolemaic hypothesis which it superseded, was to 'save the appearances', to enable calculation and prediction of the observed motions of the stars and planets without regard to their hypothetical actual behaviour. So long as the heliocentric hypothesis was used and interpreted in such a way, it would be assessed purely on mathematical and empirical grounds; the distinction between mathematical and metaphysical truth served to ensure that the validity of the heliocentric hypothesis was of such a sort that it could not conflict with the physical-cum-metaphysical truth of the loose geocentrism that had strong theological backing at the time. Any defence of the heliocentric hypothesis that claimed it to have physical validity thereby transferred the hypothesis outside of what were taken to be the bounds of science; once outside, it would then be subject to assessment on explicitly non-scientific grounds.

A stronger and more explicit emphasis on the methodological constitution of science is typical of modern positivism. To a considerable extent, this emphasis marks a reaction against the dogmatic realism typical of much physical science during the last century. Adherence to Newtonian mechanics was so strong in scientific circles around the middle of the nineteenth century that it was possible to define scientific explanation in purely substantive terms, as consisting precisely in the reduction of complex observed phenomena to the principles of Newtonian mechanics⁷. This tendency was both a product of and an ongoing stimulant to the overextension of Newtonian principles, the same overextension that came eventually to hinder the progress of

physics until the principles were recast in an empirically meaningful and hence limited form--after which, of course, they eventually came to be seen as approximations to limiting cases of the more general principles of relativity theory and quantum mechanics. The recasting of Newtonian principles, and the development of new ones, required far closer attention than had been given previously to the operational specification of any concepts that were used; concurrently, the manipulation of these new and revised concepts required, partly on account of their (then) counter-intuitiveness, a more rigorous commitment to their logical implications. It was only by such rigorous commitment to the logical and operational apparatus of research that the non-metaphysical (i.e., not overextended, hence empirically warranted and significant) components of Newtonian mechanics could be identified and retained, the metaphysical husk discarded, and newer more generally adequate principles developed. This necessity for a methodological emphasis in the successful practice of physics at the time made apparent the general logical appropriateness of such an emphasis. The detailed construction and development of an unambiguous logical framework for science which resulted from such recognition comprises the main original and systematic contribution of logical positivism to the tradition of positivist thought. For a tradition which has always valued formalisms, this contribution is a significant one.

The unity of science as discussed by Kolakowski is a consequence jointly of observationalism and of the emphasis on methodology, insofar as these two are characteristic of any positivistic science. In modern positivism, the unity of science is a unity of logical methods and of data language. Theoretical unity or unity of basic

explanatory principles is an open and broadly empirical question. To maintain otherwise would be to repeat the error of Helmholtz and du Bois-Reymond (see footnote 7 to this chapter).

The final subordinate or derived characteristic of positivism--these are all, obviously, closely interrelated--concerns the goal of science, or what we can expect science to provide us. The goal of science cannot, on positivist criteria, be the pursuit or attainment of truth. That goal, then, must be related to more modest and non-transcendent considerations, such as Mach's principle of economy of expression, or power to predict new phenomena, or perhaps practical utility. That is, at the most general level science and scientific activity can only, as suggested above, be justified instrumentally. A realist approach to science does not of course necessarily disdain instrumental considerations, but separates them from, even if it does not subordinate them to, the pursuit and acquisition of truth.

These four characteristics--repudiation of metaphysics, commitment to observationalism, emphasis on methodology and logic, and a broadly instrumentalist conception of science--are offered as providing a general characterization of positivism and as highlighting the contrast between positivism and realism. The four characteristics are listed roughly in descending order of importance to positivist thought. The relationship of these to phenomenalism and the unity of science, in Kolakowski's treatment of positivism, has been discussed. Nominalism and the fact-value distinction have not been treated here. Nominalism can be subsumed along with phenomenalism under observationalism, insofar as it avoids having metaphysical implications about the permissible forms of theoretical laws. The fact-value distinction

is not specific to positivism, and is equally applicable to most forms of scientific realism. Comte's positivism has not been treated because, despite his advocacy of a kind of observationalism, there is enough basic divergence between his position--which, according to the dichotomy advanced here, is in its central social aspects a 'realist' one--and that of scientific positivism that assimilation of the two would be a disservice to both.

It is clear that neither positivism nor realism as characterized here constitutes a theory or a set of doctrines, whether about the content of specific scientific theories or about the nature or form of reality or about anything whatever. Rather, they are divergent convictions about the possible scope of scientific theories. Neither, for that matter, do positivism and realism directly imply different strictures on how research should be done. Rather, they lead to different estimates of the status of the methodological considerations that guide that research--usually, in contemporary science, the same methodological considerations, whether the scientists making use of them are themselves realists or positivists. Thus, positivists and realists, such as Bohr and Einstein, can do very similar and fully compatible research; and while they may have utterly divergent convictions about the meaningfulness of any attempt to attain an approximation to the final truth through such research, their differences assume central importance only in their informal debates and popular writings.

Often, nonetheless, there may indeed be significant differences between the two orientations, particularly in the interpretation of scientific research. Cogent objections can then be advanced against each orientation by proponents of the other.

The objection that positivists can bring against realism is that the factual or empirical content of a scientific theory is in no way increased by tacking on to the theory a belief that it is 'true', in any sense apart from or additional to its empirically ascertained validity. On the contrary, the theory's factual content is in practice more likely thereby to be reduced. The extension of the domain of applicability of scientific theories beyond that point to which their observational warrant properly extends (which seems to be the consequence of treating them as 'really true') can lead and has led to gross errors. It was just such errors, arising from the empirically unwarranted universal extension of the concepts central to Newtonian mechanics, that led to a crisis in nineteenth century physics, stifling physical research until the conceptual excesses were rectified. This argument against realism and in favour of positivism is sufficiently familiar that it need not be elaborated; its validity is almost unquestioned.

The objection that realists can bring against positivism is twofold. First is the claim, illustrated by the quotation from Planck, that belief in the reality of a conceptual schema is necessary as a basis on which scientists can undergo the toil and strain of constructing wide ranging theories, defending them against opposition, and in the teeth of such opposition extending them in the discovery and interpretation of new phenomena. This defence of realism as, in effect, a heuristic principle, thus constitutes in Planck's formulation of it a claim about the psychology of the creative process. Consequently, it could in principle be investigated experimentally, although the attempt to do so would undoubtedly encounter incalculable difficulties. If one attempts to test Planck's principle with examples taken from the history of science, it re-

ceives at best partial confirmation. Some scientists have been positivists and other, perhaps greater in number, have been realists; many seem to alternate between the two orientations; both positivists and realists can be found among the greatest scientists in history. It is not possible, therefore, to maintain the heuristic justification of realism as a universal principle, admitting of no exceptions; although it may certainly be true that a realist world-picture is a psychological necessity at least for many scientists.

The second objection is a related one, but is of a conceptual rather than a broadly empirical nature; it is a less familiar point, and thus requires a bit fuller explanation. Positivism erects a stipulative ceiling on the capacity of science to explain the world, and even if scientists respect this ceiling, it is likely that others will not. The ceiling is imposed, of course, by the stricture against making scientific explanations refer to physical reality as such, separate and apart from specific controlled observations of it. This ceiling on scientific explanation is much the same as was imposed by medieval scholastic philosophy, both Thomistic and Averroist; it is the same limitation that Osiander insinuated into Copernicanism⁸ and that the church forced on Galileo. That it is recognized as the implication of modern positivism is attested to by the ready acceptance of logical positivism by metaphysically sophisticated transcendentalist philosophers such as Jacques Maritain and Ernst Cassirer.

The metaphysician can in effect say to the positivist scientist: "I will not try to tell you anything about the behaviour of electrons if you will not try to tell me anything about the nature of reality. Discerning the nature and structure of reality

is my job; conducting experiments and constructing empirically limited theories---without drawing any universal or metaphysical implications from them---is yours. I may indeed make reference to your empirical findings as illustrating a principle about the construction of the real world, and in so doing I will explain the significance of your results. None of this will constitute an encroachment on your domain, however, because I will not attempt to predict or stipulate in advance the particular empirical results which you will find; as Duhem (1914) has already shown, metaphysical principles never uniquely imply empirical findings in any case."

The positivist scientist must, whether willingly (as in the case of Duhem) or unwillingly (as in the case of Frank), accept this division of labour. The most he can say by way of demurral is that the metaphysical principles which the philosopher will erect will be empirically meaningless. However, the metaphysician has ex hypothesi accepted this proviso at the outset, and is not disturbed by it since any empiricist criterion of meaningfulness is, as indicated previously, purely stipulative. The metaphysician merely restricts himself to a richer kind of meaning and truth that is not empirical. Furthermore, even this restriction has little force, for while the metaphysician cannot draw empirical implications from his philosophical principles, there is nothing to stop him from drawing pragmatic ones, that is, implications concerning the way we can best assume the world to be in charting our daily actions. Thus, with both the most general and the most specific interpretations of scientific theories, as well as reality itself, all marked as his province, the metaphysician is barred only from the experimental laboratory. The positivist scientist is barred from everywhere else.

The realist scientist feels that this arrangement and division of labour is both overly restrictive and excessively cosy. He may feel that the behaviour of electrons has something to do with the nature of reality if anything does (and that he should be involved in deciding whether it does or not), and that he is therefore better qualified to say something about that reality than anyone whose sole qualifications are metaphysical ones. Positivism thus seems to the realist to be abrogating the power and the responsibility of science to discover the truth about nature and surrendering that power and responsibility to those who, even if eager, are unqualified to exercise it.

It must be said that both sets of objections, against positivism and realism alike, have considerable force. The positivist objections against realism seem clearly to have greater logical significance, while the realist objections against positivism have, perhaps, greater pragmatic and systemic significance. The positivists, nevertheless, have somewhat the better time of it in such a controversy, for only their objections appear incontrovertible. A logical purist could shrug off the realist objections as being irrelevant to the constitution of science, while few scientists of any persuasion would care to deny the claims of logic. It would seem that if one had to make an unequivocal choice between the two orientations, one would have to choose between maximizing systematic richness and maximizing logical rigour; between emphasizing intuitive and emphasizing strictly empirical significance; between the danger of developing overextended and empirically degenerating conceptualizations and the complementary danger of 'crippling the flight of the imagination' of many of the 'leading spirits' of science. It would not be a particularly happy choice.

It is not, however, mandatory that the choice be made in so uncompromising a manner, and there is little basis for maintaining that it has been regularly made in such a manner in the history of science. The key to the resolution of the conflicting claims of positivism and realism is the recognition that the strengths and weaknesses of each position are complementary, and have therefore complementary degrees of relevance during different stages of scientific inquiry. The weaknesses of realism are particularly disruptive of scientific progress at the same time, or under the same conditions, as the strengths of positivism are most conducive to such progress, and vice versa. In examining the potentialities for an interplay and alternation between positivism and realism in the conduct of scientific inquiry, we will be able to see these strengths and weaknesses in a somewhat broader perspective.

II. The Reconciliation of Positivism and Realism.

Two Types of Assessment of Scientific Theories.

Scientific theories, and even global scientific systems, frequently begin and end in periods of intense controversy concerning the appropriateness or meaningfulness of the concepts fundamental to the theory. In between these terminal periods, this kind of controversy is likely to diminish, and controversy and research both centre more typically on the application and extension of the theory in accounting for the range of events which it was designed or later extended to explain. At the beginning and the end, that is, it is customary to ask, in various ways, whether the explanatory and descriptive concepts used in the theory make sense. In between (assuming the theory survives the initial stage of questioning) it is more customary to ask, in various ways, how great a range of events the theory can be made to account for. To put it a third,

still simpler way, at the beginning and the end it is a very relevant question to ask, "Why should this (the proposed theoretical account) count as an explanation?" In the middle, it is more relevant to ask, "How much can this come to explain?" or "How can this theory be applied to the explanation of that phenomenon?"

The classic example of this shift of focus, as of so many other features of scientific inquiry, is Newtonian or classical mechanics. When Newton published the Principia Mathematica in 1686, there was no question that his account provided an excellent mathematical fit to the observed events which it sought to account for, nor that parametric predictions of these events could be rigorously derived from an impressively small set of postulates. Controversy did not turn on these features of his theory. Instead, and in advance of any systematic attempt to try out Newton's theory in the solution of other difficult problems in physics, controversy centred on the admissibility into science of some of the basic concepts involved in his theory. His conceptualizations of attractive and repulsive forces, gravitational attraction, action-at-a-distance in general, absolute space and time, all received widespread criticism. Use of these concepts in physical theory was quite widely regarded as tantamount to a reintroduction of medieval occultisms into natural science (see Koyré, 1957, for an account of the controversy). Newton was able to ride out the storm, partly by agreeing with his opponents that many of these concepts would indeed qualify as occultisms if they were taken to have literal meaning, if 'gravity', for instance, was taken to refer to a specific but unknowable quality; but on the contrary, Newton insisted that no such hypostatization was any part of his intention, that his principles were merely 'mathematical' rather than 'physical'⁹.

The provisional, and hence unobjectionable, character of Newtonian physical concepts having thus been established (to the satisfaction of some critics at least¹⁰), the way was clear for such concepts to be further assessed and evaluated on the basis of their systematic application, with the need for separate analytic justification for their use gradually diminishing. Evaluation of them on the basis of their systematic application, of course, involved their extension and adaptation to more and more diverse phenomena. As the Newtonian system was revised and successfully extended to cover observational and predictive astronomy, electricity, pneumatics, hydraulics, heat and heat transfer, physical chemistry, and other previously separate or nonexistent fields of inquiry, the meaningfulness and validity of the fundamental concepts involved came to be taken more and more for granted. Questioning of the status of concepts such as attraction and repulsion came to seem little more than a narrowly academic exercise inasmuch as the interrelated set of theories based on such concepts was having such unprecedented success in extending man's understanding of the physical world. Eventually, as described previously (and see footnote 7 to this chapter), the fundamental concepts of attraction and repulsion came to seem not only acceptable within science, but absolutely essential to its successful enterprise.

Finally, of course--the story is well known--latter day Newtonian theories began to encounter more and more serious difficulties. Anomalous findings such as unexplained perturbations in the orbit of Mercury and the failure of the Michelson-Morley experiment to locate evidence of an ether drift combined with incomprehensible theoretical predictions such as those concerning black body radiation to produce

a situation in which 'Newtonian' theory (most of it undreamt of by Newton) no longer seemed able to provide a trustworthy guide in the investigation of nature. Having provided the basis for physical science for two hundred years, and an almost unquestioned basis for over a hundred, it had finally begun to show its limitations. That it had any such limitations was a great shock to many scientists and scientific commentators, and provoked a reassessment of the status of scientific theories and of the concepts implicated in them, that is still going on. Attempts to preserve what was still valid or valuable in Newtonian theory led some physicists particularly to reanalyse some of the central concepts in the theory and to conclude that in their customary universal form they were misleading--sufficiently misleading that reliance on them was in large part responsible for what was coming to seem a crisis in physical explanation. Reformulation of these concepts (especially those relating to space and time) in more limited and empirically warranted forms was instrumental in the development of the theories--relativity theory and quantum mechanics--that accounted for many of the anomalies and eventually replaced Newtonian mechanics altogether.

The reception and later career of Wundt's new structuralist psychology displayed a similar pattern, although on a much smaller scale. The scale was smaller both because Wundt's innovations were not so great as Newton's and because the resulting theory did not have such wide applications. When Wundt first proposed his system of structural psychology in 1874, he faced opposition from the proponents of Comtean positivism (who, long before Watson, repudiated all introspection on much the same grounds as the latter), from the proponents of German phenomenology (who, long before Wertheimer,

repudiated analytic introspection on much the same grounds as the latter), and indeed from the vast majority of German philosophers (who followed Kant or Hering, both of whom denied the possibility of an experimental use of introspection¹¹). That Wundt was able to proceed and gather support for his system in spite of these objections was due to three features of his system and of the way he presented it. First, as the experimental background to his psychological system he appealed to the thriving science of experimental physiology rather than to the already extant introspectionist tradition which was dominated by phenomenologists and Cartesians (it was the latter to whom Comte's criticisms were chiefly directed). Second, consonant with the character of German philosophy at the time, Wundt's overall classification of the contents of consciousness was respectably a priori and speculative; the detailed introspective experiments which followed provided the precise details concerning the structure of consciousness but were not, initially at any rate, claimed to reveal that basic structure itself. Third, and relatedly, the elements into which he decomposed conscious experience were, if not widely adopted in German philosophy at the time, at least familiar and relatively acceptable due to their source in the well-established tradition of British empiricism. The first of these features served in part to remove Wundt's system from the ambit of its harshest potential critics, while the second and third served to reduce or mitigate its innovative character. Together, these three features enabled Wundt to bypass conceptual objections to his enterprise by staking out a small and previously unclaimed area of scientific investigation as his own. The specific means whereby the respectability of his enterprise was established were different from those employed by (and for) Newton, but the general procedure was the

same: to gain time for the system to be developed and extended by minimizing its revolutionary or otherwise unacceptable implications.

As Wundt's system became more extended and ramified, both through his own publications and through those of his students and colleagues, the need for defensiveness concerning its propriety or possibility diminished and the detailed structure of the system became of primary concern. As a result, the area which it encompassed came to be seen as identical with the reaches of experimental psychology altogether. Eventually, when other investigators, working in other contexts, became interested in matters which Wundt's particular structural approach could not adequately incorporate--imageless thought, perceptual wholes (Gestalten), animal behaviour, etc.--and of which structural psychology, because of its predominance, was seen as hindering the active investigation, the debate over the applicability and meaningfulness of Wundt's general approach began again, in renewed and intensified form. In the ensuing controversy Wundtian structural psychology withered away almost entirely, and, particularly within the geographical limitations of American psychology, introspective psychology in general lost much of its credibility.

In behaviourist psychology, too, the same kind of alternation is visible, although because of the particular exigencies of behaviourist research, described in the last chapter, the shifting of focus was not so strong or so clear in behaviourist psychology as in the other two examples cited. Conceptual analyses and thinly veiled polemics advocating the focusing of attention on strictly observable behaviour began with Cattell (1904) and Meyer (1911) and continued through Watson (1913a, 1913b), Weiss (1925), Hunter (1928), and many others. On the other side, attempted refutations or disparagements of the behaviourist

approach were made by Lovejoy (1923), Roback (1923), Broad (1925), and, again, many others. By the late 1920s such external and general criticisms were becoming less frequent, partly perhaps because of their total lack of effect. Throughout the 1930s and 1940s conceptual analyses and methodological polemics were clustered more within behaviourism, serving principally as ammunition in the rivalry between various behaviourist schools. Also in this period however, as described in the last chapter, a great deal of detailed and precise experimentation was carried out in support of the positions of the various schools. Such experimentation was relatively free from concern with the conceptual basis of any particular form of behaviourism, or of behaviourism in general, and was addressed to specific theoretical problems, in much the same way as characterized the middle periods of the careers of the other two scientific systems cited. It is difficult to state with any precision when the second period of detailed conceptual critiques of behaviourism got underway, but as indicated in Chapter 1 this period may conveniently be said to date from publication of the volume Modern Learning Theory (Estes et al., 1954); the second period has continued since that date with the eventual results described in Chapter 1, so that by now behaviourism is, though much more than a memory, much less still than a hegemony.

This alternation of critical focus from theoretical concepts to theoretical extension and application and back again is not particularly surprising, and can be accounted for, in a loose sense at least, fairly easily. If a new theory requires new ways for looking at or interrelating observed events, if it asserts the relevance to an already specified problem area of hitherto ignored or unrecognized phenomena, if in general it requires a reconceptualization of that part of the world to which it is addressed, then its sheer novelty will

ensure that it initially receives attention qua novelty rather than strictly qua theory. It will be considered as a novelty first, and to the degree that the fundamental concepts involved in it are strange ones, to that degree they will require exposure and familiarization as concepts before they are utilized within the theoretical structure. In advance of further elaboration of the theory however, it may seem entirely questionable whether it is worth the effort required to assimilate the new concepts, whether their strangeness is due simply to their newness or rather to their general inappropriateness for dealing with familiar problems. The question of the appropriateness of the concepts central to the theory is not altogether an empirical question. That is, it is perhaps conceivable that such questions of appropriateness could in principle be answered by elaborating every proposed theoretical structure and comparing the outcomes. But in a world with finite human and scientific resources any such exhaustive procedure is impossible. Hence, the question of whether or not it is meaningless or a waste of time to begin working with a proposed new theory must be answered before further theoretical elaboration and detailed testing takes place, to the satisfaction at least of enough scientists to take up the theory and continue trying to show its merits. Thus, to the degree that the concepts central to a proposed new theory are unfamiliar ones, critical analysis of the concepts is a necessary precursor to their experimental and theoretical elaboration.

If, by whatever means, a theory passes its first conceptual test, then for those who have accepted it on this basis further conceptual justification for it will be relatively unnecessary; relatively unnecessary only, for such justification may continue to be necessary in presenting the theory to critics. But for those

working inside the theory--those, that is, for whom questions of the meaningfulness or appropriateness of the theoretical concepts have been resolved, dismissed, or set aside--such justification need play no part in their application and testing of the theory. Such application and testing consists in using the theory to interpret and account for the world, to answer the kinds of questions which it was designed to answer. Thus, the success of the Newtonian physical theory did not consist in demonstrating or postulating the existence of rigid bodies, Euclidean space, rectilinear motion, or principles of attraction and repulsion; the admissibility of these concepts and postulates was a bitterly disputed precondition of the theory's evaluation. Once the existence of these entities and forces had been provisionally assumed, the success of the theory consisted in showing how the principles of attraction and repulsion could account for the rectilinear motion of rigid bodies in Euclidean space. Such achievements are the goal of theory, the goal of scientific inquiry as such, and can be pursued autonomously once the admissibility of the concepts used in the theory has been established.

The second period of conceptual analysis, toward the end of the career of a scientific theory, can be accounted for in a similar manner. To begin with, the end of one theory frequently overlaps with the beginning of another which replaces it, and if the new theory requires another reconceptualization it will provoke conceptual analysis and examination in the way described above. In addition, if a well supported and useful theory begins to encounter numerous serious anomalies, to the extent that the theory cannot consistently be upheld, it might signal an unacceptable loss of economy to reject the theory outright. There may be nothing of comparable power to replace it with,

at least until a new theory has been developed to an equivalent extent. Thus, it becomes desirable to determine the bounds within which the old theory can continue to function and beyond which it is invalid. Determination of these bounds within the limits of the old theory itself involves recasting it so that it does not extend to the types of situation in which it is inapplicable; it involves, therefore, a reformulation of the theoretical concepts in such a way as to limit their scope. Finally, a reanalysis of the foundations of an old theory might be undertaken as part of an effort to repudiate it, to show that the eventual failures of the old theory were inherent in it from the beginning, that, as Watson characterized structuralist psychology, it "was founded upon the wrong hypotheses." All three of these bases for analysis of a declining theory overlap and can be considered together. On the most general level, we can say that when a scientific theory is doing its job, leading to successful investigations of nature, it is relatively unnecessary to question its foundations; when it stops or fails to do its job, or before it is allowed to begin, questioning its basis is almost essential.

This account of the basis for the shift in focus from the theory itself to that part of the world which the theory addresses and back again serves also to suggest the circumstances in which the examination of some theories will not display the shift. The amount of justification for the fundamental concepts of a new theory which will be required depends on the extent to which these concepts are unfamiliar or incompatible with those hitherto accepted. Thus, a new theory which does not require any reconceptualization of a part of nature can be assessed more or less immediately on the basis of its elaboration and empirical support. Alternative theories within a

given theoretical matrix are of this sort. Thus, within the Hullian framework, assessment of Mowrer's two factor theory (Mowrer, 1947) could be carried out on a strictly empirical basis from the beginning; those psychologists who were concerned with the theory understood what Mowrer was talking about, and accepted his theoretical terms as meaningful, without any specific justification or explanation of them. At the other extreme, a theory might require so great a reconceptualization of its subject matter that almost all scientists concerned with it reject the theory as outlandish, while preserving, perhaps, whatever component of it can be fitted in with current conceptions. Such was the fate of Fechner's psychophysical theory (Fechner, 1851, 1860); the psychophysical methods which he developed were assimilated into the mainstream of early experimental psychology, while the theory itself was universally dismissed as unscientific and mystical. Unfortunately, it is not obviously apparent how one could specify in advance how much 'outlandishness' a new theory will be allowed to have before it is rejected out of hand. There is a rough sense in which a proposed theory will have to be at least somewhat compatible with the general conceptual framework of contemporary science for it to be given serious consideration; but determination of whether or not a given theory is or is not sufficiently compatible can perhaps be done only after the fact. Newton's and Fechner's theories were objectionable to their respective contemporaries on much the same basis, to wit, that the fundamental concepts involved in them were mystical, obscurantist, empirically meaningless, and in general not up to current scientific standards.

The Context of Construction and the Context of Reconstruction.

For convenience of reference, I propose to lable the contexts

in which the two different kinds of questions described above are asked of a theory as the 'context of construction' and the 'context of reconstruction'. The context of construction is that in which theories are elaborated and tested in terms of how well they make contact with nature, how well they fulfill their predictive and explanatory tasks. The context of reconstruction is that in which the terms, concepts, and variables out of which the theory is built up are subjected to searching critical examination, in order to establish or disestablish them as appropriate or meaningful.

This terminological convention need not be taken too seriously, but it is at least convenient in the present discussion. Furthermore, the terms have been chosen with some care. 'Construction' signifies a building-up and progressive development of a theory, a house, or whatever; and 'reconstruction' signifies an at least partial tearing down and replacement or strengthening of the foundations, as a preliminary to further building. Scientific work undertaken within these two contexts is often loosely separated in time, for the reasons given. The context of construction is primary while a theory is being extended and elaborated, and the context of reconstruction is primary while a theory is being initially examined and finally abandoned. Work done within the two contexts certainly interpenetrates in time however, and may well be co-extensive with only variations in relative emphasis throughout the life of a theory.

It can be seen that the proposed distinction between the context of reconstruction cuts across the well-known distinction, dating back at least to Herschel (1830), between the context of discovery and the context of justification. The present distinction is not so sharp as that between the contexts of discovery and justification, however,

because the criteria for judgment of a theory are by no means completely separated or mutually exclusive within the two proposed contexts. They are clearly separable, nonetheless, inasmuch as different questions are asked in the different contexts. If one is pursuing a particular elaboration of an already well attested theory it is relevant, but usually only tangentially relevant, to be given information concerning the logical and empirical status of the concepts fundamental to the theory. Similarly, if one is examining the elements of a theory to see if they have, for instance, unambiguous empirical referents, then it is relevant, but usually only tangentially relevant, to be told that the theory accounts for a given phenomenon with such-and-such a degree of success. The two contexts are related hierarchically, in that the context of reconstruction is or should be subordinate to the context of construction, because it is in the latter context that the development of science as such takes place. The context of reconstruction comes into its own most forcefully during the decline of a scientific theory, when the conceptual analysis of the theory is undertaken in order to determine why it is that questions asked in the context of construction are no longer receiving satisfactory answers.

The Differential Relevance of Realism and Positivism
to the Contexts of Construction and Reconstruction.

The distinction between the context of construction and the context of reconstruction, and an appreciation of the different tasks appropriate to the two contexts, makes possible a resolution of the conflicting claims of positivism and realism in the conduct of science. In brief, the position advanced here is that a realist orientation towards scientific theories and what they account for is most conducive to scientific progress within the context of construction. Conversely,

a positivist orientation is most conducive to scientific progress within the context of reconstruction.

A summary justification for this position is implicit within what has already been said in the description of the two orientations. In the context of construction, attention is focused on (what is taken to be) the world; there is greatest conceptual economy in assuming that the world autonomously possesses those characteristics which are attributed to it by the theory and which are progressively elaborated in the course of scientific discovery. Furthermore, in the context of construction, there is no reason to assume otherwise about the status of such theoretically attributed characteristics; to do so would only distract attention from the task at hand, that of further determining the character and structure of the world through elaboration, testing, and revision of the theory. In the context of reconstruction, on the other hand, attention is focused on the variables and concepts of the theory, primarily as components of the theory and only secondarily as attributes of the world. There is greatest conceptual economy in examining these as they occur, without assuming that they either have or do not have a universal external reference independent of their specified observational content. Furthermore, within the context of reconstruction, any assumption about the 'objective' (i.e., 'real') reference or lack thereof of such variables and concepts would, as in the previous parallel case, distract attention from the task at hand, which in this case involves explicating their logical implications and observational content.

To put it another way, relating to the pragmatics of science rather than to its semantics, a realist orientation towards a particular

scientific theory can be expected to encourage tenacity in the maintaining of that theory in the face of potentially disconfirming evidence, and to promote commitment to the general validity of the theory in its future application to specific problems. In the elaboration of a theory which has already received some development, such tenacity and commitment will often be rewarded. A positivist orientation, conversely, can be expected to encourage flexibility and lack of full commitment in the assessment of and choice between theories, a flexibility that is sorely needed when theories are being broken down and examined piece by piece. Such flexibility, however, may reduce to vacillation, and withholding of commitment to nit-picking, as a science incorporating one or more major theories proceeds from strength to strength in the successful investigation of nature. Equally, commitment to the value and validity of a theory may be arbitrary and capricious when a new point of view incorporating it is first expressed; even worse, tenacity may degenerate to dogmatism as the breakdown of a scientific synthesis indicates to an uncommitted observer that something is basically, i.e., conceptually, wrong with the theoretical position.

The differential relevance of the two orientations, in short, establishes the need for an alternation between them, corresponding to the alternation between the contexts of construction and reconstruction. What seems to be most called for, that is, is a progressive alternation of realism and positivism in the conduct of science, with realism predominating in the context of construction and positivism predominating in the context of reconstruction. Furthermore, something at least resembling such an alternation, as a function of different problems faced at different times, is indeed apparent in the examples of the careers of scientific systems--Newton's and Wundt's--cited above.

A more detailed justification for this position concerning the relationship between realism and positivism will require a further examination of modern positivism. It will hinge largely on showing that, regardless of our preferences in the matter, a consistently positivist approach is almost impossible to maintain in the context of construction. The justification for realism in the context of construction will, correspondingly, be pragmatic rather than ontological. It will proceed, that is, not by attempting to validate or defend the realist conviction that there is a real world with determinable characteristics existing independent of our experience of it, but by showing how the assumption of the existence of such a world can facilitate scientific advance. The analysis will have to centre on the fine structure and detailed implementation of a positivist orientation in science, for the specific features that limit the applicability of positivism are not such as show up in summary or programmatic statements of the position. The analysis will therefore be directed for the most part to the characteristics of modern logical positivism and related movements, for it is only through these that the implementation of positivism has become sufficiently sophisticated and detailed that it can be examined with anything approaching the necessary precision. This circumstance is also fortunate of course, in that it was the modern and sophisticated forms of positivism that eventually came to be implicated in behaviourism, and so it is with these that we would in any case be concerned.

The description and analysis of positivism has already progressed far enough, however, that it can begin to be applied to the development of behaviourism, especially to behaviourism in its initial versions before it became associated with the formal movement of

logical positivism. Making some use of the descriptive vocabulary that has been built up in this chapter, therefore, the next will describe the background to and the initial emergence of behaviourism; it will show how right from the beginning, within the context of reconstruction, the incorporation of positivism in behaviourism was significantly variant from the usual and appropriate pattern as described here.

Chapter 4

Behaviourism's Background: The Instigation to Behaviourism in Studies of Animal Behaviour

Two features of behaviourism were cited as central to the movement in Chapter 2. The first was the repudiation of unobservable entities and processes--particularly the mind and consciousness, but by extension others as well; it was this repudiation that, as we have seen, more than anything else marked the emergence of behaviourism. The second was the adherence to explicit decision procedures as the basis for evaluating scientific statements, hypotheses, and theories, and as sufficiently establishing thereby the scientific and presumably progressive character of behaviourist theorizing. In Chapter 3 it was pointed out that these two features also generally typify different ways of implementing a positivist orientation toward the practice of science. In behaviourism as--at least in modern times--in the physical sciences, the repudiation of unobservables was the initial means for the implementation of positivism, and the adherence to explicit decision procedures was the later, more sophisticated means. We can say roughly that the repudiation of unobservables characterized the positivist orientation of behaviourism from the beginnings of the movement until the development of 'neobehaviourism' in the late 1920s and early 1930s, and that the adherence to explicit decision procedures characterized it thereafter.

Chapter 5 will deal with the use of decision procedures in neobehaviourism and generally in science, both by briefly documenting their popularity in psychology and by analysing their structural potentialities and limitations at some length. The present chapter

will deal with the circumstances which led to the initial emergence of behaviourism in the early part of this century; it will thus be concerned with what was called in Chapter 3 the 'context of reconstruction', and with the inclination toward positivism which can be expected to arise within that context. It will consider, that is, the circumstances that led to behaviourism's being initially based on or nearly identical with an objectivist and positivist conception of psychology, characterized by the repudiation of the mental and all other unobservables, and distinguished from the rest of psychology primarily by its methodological stance rather than by its subject matter or its substantive principles. The discussion will be fairly extensive for a number of reasons: first, a discussion of the circumstances of behaviourism's birth is obviously germane to a general examination of the movement; second, it is important to emphasize the specificity and even uniqueness of these circumstances; and third, some of the effects of behaviourism on the later practice of psychology make sense only in comparison with the dominant trends of the psychological tradition which stimulated the emergence of behaviourism and which behaviourism supplanted.

To consider the birth of behaviourism we must look to the early history of comparative psychology. The circumstances which led to the establishment of behaviourism, the repudiation of unobservables, and the institution of positivism--all three labels referring to the same complex event--arose around the turn of the present century, particularly in American psychological laboratories. They arose in the context of problems in the study of animal behaviour, Thorndike's and Watson's initial field of interest. What happened in brief was that in such studies, the growth of sophisticated methods of

experimentation and experimental control was proceeding faster than, or in a different direction from, growth of the functionalist and evolutionary conceptual framework on which the studies were based. As a result, the evolutionary analyses applied to the behavioural observations which were the immediate results of the studies came to seem outmoded and irrelevant, an unnecessary gloss on the increasingly precise observations which were being made. The observations and experimental results themselves came to seem the only valuable aspects of the research programme, and quite independent of their dubious theoretical interpretation. In this situation, the suggestion that attention might better be confined to mere data, with the theoretical interpretations repudiated on principle, was an obvious, welcome, and not altogether unreasonable one. The widening gap between data and theoretical interpretation of data which seemed to justify this separation of the two was not, however, an inevitable consequence of research on animal behaviour at the time. To understand as far as possible how this gap developed, and what alternatives to it existed, it is necessary to go back a further step and look in some selective detail at the development of this research on animal behaviour from which behaviourism sprung.

I. The Conceptual Development of Comparative

Psychology: 1882-1901.

Animal behaviour became of interest to psychology mainly through the development of comparative psychology. Studies of animal behaviour had previously been undertaken, if at all, as part of natural history or in connection with the practical demands of husbandry and selective breeding. Studies made on these bases, while occasionally acute, lacked any systematic theoretical basis for the

selection and interpretation of phenomena to be studied. Comparative psychology, based on Darwinian evolutionary theory, provided such a theoretical basis. The goal of comparative psychology, from the time of its first elucidation by Romanes (1882), was to demonstrate the qualitative continuity of inferred adaptive capacity and psychic processes throughout the animal kingdom, and complementarily, to trace the stages of evolutionary development of such capacities and processes to their culmination, frequently in man. The capacities and processes of principal interest were what were considered to be the higher mental or psychological ones. That is, they were those manifested in the plasticity, variability, and modifiability of behaviour, and often termed 'cognition', 'reason', etc., in contradistinction to those specific structural-cum-behavioural ones manifested in the particular abilities (for flying, burrowing, etc.) of different animals. This choice of emphasis was in part anthropocentric; the desire was to explicate the kinship between the animals and man specifically with regard to man's highest and most 'distinctly human' faculties, those to do with rational thought and consciousness. The choice could also be justified non-anthropocentrically however, since capacities for varying and adapting behaviour could be considered more fundamental than specific behavioural capacities, at least in the sense that the former, by being a step removed from specific behaviours, can be studied as such throughout the animal kingdom and hence can be expected to show most clearly what evolutionary progression, if any, occurs. One can compare turtles and raccoons, say, with respect to their abilities for exhibiting response perseverance or for grasping three-term relations, with the hope that the results may illustrate general differences in the ways that turtles and raccoons are able to function

in the world; but there is less point in comparing them with respect to their abilities for climbing trees. Or so at least it seemed.

The general method initially used to implement the programme of comparative psychology was a combination of behavioural observation, analogy, and inference. Observations of selected behaviour which could not be accounted for on the basis of instinct or reflex action--because of the behaviour's novelty in the history of the organism and its adaptive specificity to the requirements of an unfamiliar situation--served as the basis for inference of generalized adaptive capacities. These capacities were conceptualized as resulting from the action of a mind exhibiting characteristics and undergoing experiences analogous to those typical of a human mind. The method was succinctly described by Romanes.

For if I contemplate my own mind, I have an immediate cognizance of a certain flow of thoughts and feelings, which are the most ultimate things--and, indeed, the only things--of which I am cognizant. But if I contemplate Mind in other persons or organisms, I can have no such immediate cognizance of their thoughts and feelings; I can only infer the existence of such thoughts and feelings from the activities of the persons or organisms which appear to manifest them. Thus it is that by Mind we may mean either that which is subjective or that which is objective. Now throughout the present work we shall have to consider Mind as an object; and therefore it is well to remember that our only instrument of analysis is the observation of activities which we infer to be prompted by, or associated with, mental antecedents or accompaniments analogous to those of which we are directly conscious in our own subjective experience. That is to say, starting from what I know subjectively of the operations of my own individual mind, and of the activities which in my own organism these operations seem to prompt, I proceed by analogy to infer from the observable activities displayed by other organisms, the fact that certain mental operations underlie or accompany these activities (Romanes, 1884; 1969 ed., pp. 15-16).

In short, Romanes' procedure was to infer the presence of consciousness in animals in the same way that he supposedly inferred it in other humans, by analogy from his own conscious experiences. He then used

the same procedure to infer the particular states of consciousness of the animal which he was observing, that is, by considering how he would feel if he were acting in the same way the animal was acting or were exposed to the same situation as that to which the animal was exposed. The description of the animal's states of consciousness was expected to be the same sort of description as would result from introspection of the contents and structure of human consciousness, roughly the same, that is, as resulted from the experiments in analytical introspection typical of human psychology at the time.

This general approach to the study of animal psychology has become so discredited that it is difficult to assess it in anything like its own frame of reference, and difficult even to see why the attempt should be made. Such an assessment is nevertheless necessary as a basis for tracing the development of comparative psychology. Romanes' approach was also used, with modifications, by succeeding writers in comparative psychology. It has been almost universally, and facilely, repudiated on the grounds that it is impossible for us to gain access to the alleged subjective experience of other organisms (which none of these writers ever denied) and that comparative psychology based on Romanes' method of inference never made any notable theoretical or substantive advances (which is not strictly true). The central features of Romanes' inferential method have almost never been afforded a critical examination outside the previously established context of a blanket acceptance or rejection of them. What follows, therefore, is an attempt, first, to indicate what could be judged inappropriate in Romanes' method and in his conception of the task of comparative psychology, from the standpoint of one working in the same general intellectual or scientific context as Romanes; and second, to

delineate the minimal changes in that method and conception necessary to establish comparative psychology as a viable research activity. Such an attempt is not merely an exercise in cultural relativism. The necessary amount of suspension of our own standards of criticism is very limited, amounting to no more than a de-emphasis of the same strictures against the study of mind in itself which, as we have seen in Chapter 1, have already been called into question. Furthermore, the minimal changes which will be shown necessary are, to a considerable extent, the same changes that were in fact tacitly made as comparative psychology developed.

The central feature of Romanes' approach is, of course, his use of analogy; that is, it is his reconstruction of the minds of animals on the basis of analogy with the operations, structures, and subjective experiences typical of his own mind. This 'argument from analogy' depends on two major assumptions. The first is that the operations and the quality of subjective experience typical of human minds are already known, or at least can be readily determined through introspection or objective experimentation¹. The second is that the minds of animals are similar (or analogous) to the minds of humans, both with regards to their subjective experiences and with regard to their mental functions or operations. To pass judgment on these in a nutshell, the first assumption is incorrect, the second so far as it relates to subjective experiences is strictly speaking untestable but universally agreed (e.g., by Romanes, see below) to be highly dubious, and the second so far as it relates to mental operations should not be introduced as an assumption at all, as it is the same continuity hypothesis that comparative psychology was designed, and was competent, to investigate. Let us consider each of these in turn.

That the first assumption is incorrect is fairly apparent. We do not have sufficiently detailed knowledge of our own mental operations to use these operations as the basis for constructing and extending such an analogy. Even if our own mental operations and private experiences could in principle serve as the basis for constructing and utilizing the analogy, the construction would have to wait on the development of a human psychology rich enough to systematize or account for such experiences. That the operations and conscious states of the human mind are transparent to casual introspection may be termed the Cartesian fallacy (even if it was chiefly Descartes' followers, rather than Descartes himself, who committed it). It was this fallacy that bore the main brunt of Comte's and, in Britain, Maudsley's attacks on introspection, and it was as clearly a fallacy in Romanes' time as it is in ours². The detailed knowledge of human psychology necessary to eliminate dependence on the fallacy might be acquired as a result of the systematic practice of analytic or phenomenological introspection or objective experimentation; recognition of the fallacy has no implications for the way in which the remedial knowledge can best be acquired. In any case, such knowledge was not available to Romanes; nor, indeed, is it available to us. In the absence of such knowledge, neither term in the analogy--our own minds or the minds of others--can be built upon with any confidence³.

The main brunt of the criticism, however, must fall on the analogy itself, relating both to the subjective experiences and to the mental operations (cognitive functions, etc.) of non-human organisms. Here there are several points to be made. To begin with, it was generally recognized that the analogy cannot be given any independent justification. The analogy constitutes an assumption that certain

observable behaviours are accompanied by certain subjective experiences or mental operations, such experiences and operations being somewhat the same as what we humans would have (or perform) in a similar situation or when behaving similarly. However, since we have no access to these elements of inner mental life--it is a truism that the subjective experience or personal consciousness of others is closed to us--we cannot prove or even provide the slightest trace of evidence for the validity of the analogy. This much was freely admitted by Romanes and by all later writers who made use of the analogy, and accepting this was almost a precondition of the analogy's use. The position of these writers was that the analogy could be justified by its heuristic use in interpreting the mentality of animals, and that the analogy, although totally unprovable, was sanctioned by custom, as it was the same as was continually used by each of us in attributing consciousness to other persons⁴.

Now, regarding this supposed sanction by custom: the analogy assumes a large part of what comparative psychology was purportedly attempting to investigate, that is, the continuity of mental processes throughout the course of evolution. If one is concerned about the presence of consciousness and cognitive faculties in animals, and desirous of refuting the Cartesian view that animals are unconscious automata, then one must initially treat it as an open question whether animals are conscious or not. It is certainly conceivable--it was widely enough conceived--that they might not be. Since the thesis that animals are conscious was regarded as central to the evolutionary view, it was hardly appropriate to introduce the thesis as an assumption. Contrary to what Romanes claimed, the assumption of consciousness in animals is not comparable to the assumption of consciousness in other

persons, even if this latter can also be regarded as an inference (cf. footnote 4 to this chapter). If our choices are restricted either to attributing consciousness or to denying it, the alternative to the latter assumption is solipsism; the alternative to the former is nothing of the sort. The two assumptions are thus not practically equivalent. They are theoretically equivalent only in that each is utterly incapable of evidential justification; if this property were regarded as sufficient vindication for assuming consciousness in animals, it would also vindicate assuming consciousness in plants, stones, and the cosmos as a whole.

Furthermore, even if we assume that animals are conscious, in the sense of having private subjective experiences, the analogy does not in fact provide a useful guide to investigating the nature and content of their consciousness. That is, the heuristic justification of the analogy also breaks down. The analogy requires not only that animals be conscious, but also that their consciousness be similar to ours. By use of the analogy we can attribute to animals only the same feelings as we would have in a given situation, or the same mental operations as we would perform. We are therefore unable to deal either with the diversity of evolved forms or with the process of evolutionary development itself.

To consider the matter of diversity: if we are to use the analogy, we are obliged to assume that every creature that can respond to a form of energy to which we also are sensitive has the same experience, upon being affected by that energy, as we do (subject to a qualification to be discussed below); as Romanes specifically stated, it is only on the basis of this assumption that we can talk about the mental lives of other creatures at all. Given, however,

nature's notorious prodigality in bringing about a wide diversity of evolved adaptive mechanisms for performing similar tasks, sensory receptors for responding to given energy forms, etc., it would seem improbable that these would be coupled with a singular parsimony with respect to subjective experience. This is an a priori argument, admittedly, but then it is an a priori position that it is being brought against. The point is, once it is admitted as a reasonable possibility that some animals might have a different kind of private experience than we do when confronted with a given stimulus situation, then the presumed universal applicability of the analogy breaks down. But although we may grant that the analogy will sometimes fail, we do not know what the circumstances or situations are under which it will fail; hence we do not know when the analogy can successfully be applied and when it cannot.

We might attempt to resolve this specific difficulty by stipulating that morphological similarity will govern the application of the analogy, so that structurally different receptors for the perception of a given energy source, for instance, will be deemed to give rise to different forms of subjective experience. This stipulation, it should be noted, can itself be introduced only as an assumption which is in principle unjustifiable. Furthermore, it does not help very much. For one thing, it precludes any hope of making comparative psychology generally applicable throughout the animal kingdom; it prevents us, that is, from giving any consideration to any animals that are structurally dissimilar to man on the dimensions being investigated (and since all animals are structurally dissimilar to man on all dimensions to some degree, we would have to stipulate an arbitrary cutoff point beyond which we would agree not to employ the

analogy). For another, we would face the complementary difficulty to the one first mentioned, in that we would have to assume complete disuniformity of subjective experience whenever the opportunity arose. But just as the diversity of evolved forms might make us wary of postulating complete uniformity of subjective experience, so might the frequency of instances of convergent evolution make us wary of postulating complete disuniformity.

All of this applies to the subjective experiences enjoyed by diverse creatures when confronted with the same types of stimulus situation as those which we also can experience. There are, furthermore, at least some cases in which we can be quite sure that if an animal has conscious experiences at all, then those experiences are often totally dissimilar to any of ours. Some animals are sensitive to forms of energy or sources of information to which we are not--bees to the direction of polarization of light, for instance, or eels to very faint electrical currents in water--and their subjective experiences in response to these forms of stimulation are therefore in no way, so far as we can imagine, relatable to ours. In this latter case, we can at least say how far the analogy fails us, that is, utterly. In the former cases the analogy may fail us utterly, or in some respects, or not at all; but we are quite unable to find any indication as to whether, or when, or how much.

To turn to the question of development: restriction to our own experiences as a standard also, although relatedly, makes us unable to deal with the process of evolutionary development through use of the analogy. It was assumed by most writers who employed the analogy that its adequacy would decrease as a function of the phylogenetic distance separating man from any other given species. The shape of the 'function'

is, of course, not determinable, so that we cannot say how much less valid the analogy will be when applied to a cat than a monkey, a pigeon than a cat, etc. But even apart from the question of the shape of the function, this acknowledged limitation on the use of the analogy renders it of practically no comparative use. If someone steps on my toe I will experience a certain sensation of pain. If now I step on the toe or other exposed extremity of a dog, a chicken, a frog, a fish, a crab, a flatworm, and a paramecium (the particular series is not incontrovertible), all I can say about the sensations which these creatures have is that I am progressively less certain as we go through the list that what each experiences is what I experienced. I may stretch the point slightly and infer that what each of these animals feels is progressively less like what I felt, but what it is that they did feel, with mounting dissimilarity to what I felt, is completely indeterminable. Thus, the analogy cannot be used in comparing what animals at different evolutionary distances from man will feel in a given situation, nor, mutatis mutandis, in comparing what they think (or otherwise do in their minds). On the other hand, if the assumption were not made that the adequacy of the analogy deteriorates with phylogenetic distance, then we would have to assume that each of these creatures felt just what I felt, with no variation due to evolutionary divergence--an assumption which few writers would be prepared to make, and which would flatly contradict the fundamental evolutionary principle of development. But the alternatives in the comparative use of the analogy are restricted to either denying evolutionary differences or being mystified by them; in either case, the analogy cannot encompass such differences as do occur. Even less, if possible, can the analogy be used to compare the experiences of animals at approximately

equivalent evolutionary distances from man, because the assumed deterioration of the analogy applies only to the distance of non-human species from man, not from each other; this point relates back to the problem of evolutionary diversity, discussed previously. Do a cat and a dog feel the same thing in a given stimulus situation? Do an arthropod and a mollusc? We do not know of course, and presumably will never find out. The analogy, which is supposed to be the tool we use in investigating the consciousness of animals, cannot let us infer, rightly, wrongly, or indeterminately, whether the experiences of these pairs of animals are the same or different.

In summary, the analogy may well have some degree of validity in some situations; we have simply to grant this point if we are to discuss the analogy, because it cannot be established in any other way. In those cases where the analogy has some validity, we will make more accurate or valid interpretations of the animal's private mental life if we employ the analogy than if we do not; the surface appeal of the analogy is based on this consideration. However, the same kind of (slightly loose) reasoning that suggests that the analogy has some point suggests at least as strongly that the analogy will not always hold; that it will hold for some species and for some components of mental life in a given species better than for others; and that in different species where it has the same overall applicability--that is, presumably, in different species at about the same phylogenetic remove from man--it may hold differentially for different components of mental life depending on the particular species involved. Furthermore--and this is the crux of the matter--all this variability in the worth of the analogy is hidden from us and cannot be brought into the open. We can assume that the analogy deteriorates as a gross and indeterminate

function of phylogenetic distance and morphological dissimilarity, but this assumption, which can only be introduced as such, does not in itself provide any insights into the subjectivity of animals as revealed by use of the analogy. In any given application of the analogy we cannot be sure that it holds at all (although we suspect that sometimes--we do not for sure know when--it does not), how well it holds (although we suspect that it holds for some species--we do not for sure know which ones--better than for others), for what components of mental life it holds best (although we suspect that in a given species it holds better for some components--we do not for sure know which ones--better than for others). In short, the degree of validity of the analogy is not only variable but indeterminably variable. Since it is variable, we must know the sources and dimensions of variability in order to make any informed use of the analogy; but since the variability is indeterminable, we are unable to do so. If, in any situation, we employ the analogy, then whether our inferences based on it are valid, and if so to what extent, and in what respects, are all matters which we can never know.

At least some of these problems in the use of the analogy were recognized by Romanes and by succeeding writers who made use of it. They did not explore its limitations in detail, but agreed that its validity was both questionable and unanswerable, and in particular that it deteriorated as a function of phylogenetic distance. Inferences to the mental lives of ants and bees, it was agreed, could not be so well founded as inferences to the mental lives of apes and dogs. Nevertheless, it was maintained, while we may acknowledge the dubiousness of all such inferences, and especially of the former set, we must still continue to use them, even if with great caution, simply because

they comprise the only interpretive method available to us.

That is to say, if we observe an ant or a bee apparently exhibiting sympathy or rage, we must either conclude that some psychological state resembling sympathy or rage is present, or else refuse to think about the subject at all; from the observable facts there is no other inference open. Therefore, having full regard to the progressive weakening of the analogy from human to brute psychology as we recede through the animal kingdom downwards from man, still, as it is the only analogy available, I shall follow it throughout the animal series (Romanes, 1882; 1895 ed., p. 9).

While restricting our attention still to an examination of the kinds of methods which were available for Romanes to employ, we now turn to the question as to whether there was in fact any "other inference open", and how the question came to be handled by the comparative psychologists who succeeded Romanes.

If there was to be any "other inference open" than one requiring the attribution of sympathy or rage to bees, it would evidently have to be one that was less dependent on the subjectivity of the experimenter or observer. That is, the grounds for the inference of mind in other organisms could not include in any central role the experimenter's subjective experience or his reflection on his mental states because, for the two sets of reasons given above, such subjective experiences and reflection do not provide sufficient information for the construction and critical application of the analogy, whether the analogy be directed toward apes or fishes⁵.

These considerations bring us to the second part of the assumption regarding the analogy (cf. p. 140), that is, relating to the similarity of mental operations or cognitive functions in man and beast. All that was said regarding the inapplicability or indeterminable applicability of the analogy applies equally, of course, to everything that supposedly goes on inside an animal's mind. In the case of mental

operations, however, the analogy is not only more or less useless, it is also, as we shall see, irrelevant. Questions about an animal's mental operations, or more properly about its capacities for such operations, are questions about what the animal can do; and such questions can be answered on the basis of objective experimentation. In fact, Romanes himself attempted to answer them objectively. Although for the most part Romanes held true to his subjective method of interpreting the animal mind by explicit analogy from what he knew of the contents of his own mind, he was also very concerned to introduce objective (that is, publically applicable) criteria by which the inference from behaviour to mind could initially be justified. As a result, the basis for a reduction (if not outright elimination) of subjectivity was present in his own writings, and was confounded with his subjective methods.

The criterion chosen by Romanes, and retained with only slight modification by later writers, was essentially the ability to learn, or to modify behaviour selectively in response to the demands of a novel situation. The selectivity of the adaptive response was taken to differentiate learned responses from reflex ones which, while adaptive, are fixed and invariable.

It is, then, adaptive action by a living organism in cases where the inherited machinery of the nervous system does not furnish data for our prevision of what the adaptive action must necessarily be--it is only here that we recognise the objective evidence of mind...Does the organism learn to make new adjustments, or to modify old ones, in accordance with the results of its own individual experience? If it does so, the fact cannot be due merely to reflex action in the sense above described, for it is impossible that heredity can have provided in advance for innovations upon, or alterations of, its machinery during the lifetime of a particular individual (*ibid.*, pp. 4-5).

In practice, Romanes further refined this criterion so as to make it as unambiguously applicable as possible in considering specific instances

of behaviour. He did not consider it definitive however, but merely broadly descriptive, and was concerned that in certain cases it might lead to an excess either of parsimony or of generosity in the attribution and description of mind.

There was a tension in Romanes' analysis resulting from the fact that two more or less separate lines of reasoning--objective inferences based on observations of behaviour and subjective inferences based on application of the analogy--were directed toward the same goal, the description of the animal mind. Generally, although not with perfect consistency, Romanes tacitly resolved the tension in the same way as did succeeding writers, by using the objective inferences to detail the operations and capacities of the animal mind and subjective inferences to describe the animal's subjective experiences. In practice therefore, and for what will be seen to be very good reasons, the analogy from human to animal mind played much less part in the reconstruction of what the animal could do than in the attempts at describing what it felt.

The differences and the relationship (or lack of relationship) between these two forms of inference should be made clear. If a particular capacity--for recognizing size relationships, say, or for abstracting the spatial positions of objects from a single perspectival view of them⁶--is necessary for the performance of a certain action, and if the animal proves able to perform the action, then the capacity can be inferred. The procedure is not all that simple of course; it is not always an easy matter to judge whether the capacity being sought is required for the acquisition or performance of a given test action, or is only consistent with it, and the methodological development of comparative psychology after Romanes consisted largely in refining the

bases on which it could be concluded that the capacity was in each case actually required⁷.

It is obvious that such objective inferences do not provide direct contact with the inner mental life of an animal any more than subjective inferences do. They do, however, make it possible to judge that either a given set of mental operations, or a functionally equivalent set, has taken place. The degree of precision in the specification of what mental operations, if any, may be said to have taken place is dependent only on the degree of precision with which an experimental task is established (cf. the account of Thorndike's analysis, below).

Thus, the operations and functional capacities of mind can be, if not defined in terms of their behavioural consequents, at least measured and assessed in such terms. The same could not be said of subjective experience, at least so long as experience was considered, in the empiricist tradition, as something separate from action, and especially so long as the experiences being inferred were required to have the same degree of specificity as those typically considered in introspective psychology. Experience as such has no 'functional equivalents'. Neither has anything else considered as such, of course, but mental operations and abilities could be considered primarily in terms of their functional significance, while subjective experience, because of its specifically existential significance, could not. Determination of the specific quality of experience was precisely what was of interest. Thus, subjective experience could only be considered in its own right, in the way in which it actually occurred--that is to say, failing any better method, by application of the analogy. The objective criterion could, if used carefully, be used to justify the

inference that an organism being tested possessed certain capacities for action, or that it could comprehend relationships (and manifest its comprehension in action), or even that it was conscious, so long as consciousness was considered as a capacity or mechanism for the differential sensitization and direction of attention, rather than as the 'qualia' which presumably comprised its content; but the objective criterion could not, as we have seen, justify any inference relating to the subjective experience of the organism as something distinguished from its capacities or propensities for action. The objective criterion could guide the choice of what particular subjective experiences were to be inferred, but that inference itself could be justified only by prior acceptance of the analogy.

In short, subjective experiences are one thing and capacities or dispositions for adaptive behaviour are another, and the bases on which each can be inferred are separate. The inference of cognitive or other capacities can be made on the basis of behavioural evidence, that is, on the basis of behaviour which cannot be accounted for without postulating the capacity. The inference of subjective experiences analogous to those of humans requires, in addition to behavioural observations, the prior and inescapably a priori assumption that such subjective experiences occur and are, in fact, analogous to ours. Inferences concerning an animal's subjective experiences are thus not only separate from but, what is more important, irrelevant to, inferences concerning its mental operations, i.e., its capacities for solving problems, grouping like and unlike objects along prescribed dimensions, or whatever; for the latter are sufficiently manifest in observable behaviour and the former are not. It follows that the qualitative psychic or mental continuity throughout the animal

kingdom can be demonstrated (if indeed it is present), and the stages in evolutionary development traced, without reference to subjective experience; the task requires reference only to capacities, 'faculties', mental operations, etc., which, while not themselves observable, can be inferred and subjected to critical examination strictly on the basis of their observable effects. What would be left out of such an account is any description of the subjective experiences themselves, and however great that loss might be it was inevitable under the circumstances, for given the inadequacy of the analogy, and the unavailability of any alternative, such subjective experiences were in any case being introduced into the interpretive account only by what amounted to a fiat.

Thus, the way to overcome Romanes' problem of there being "no other inference open" than one requiring the attribution of sympathy or rage to bees is to recognize, first, that there are indeed other inferences open, inferences relating to what the bee is capable of doing (including, from the construction given, what it is capable of 'thinking', if anything); and second, that the inference of sympathy or rage is on Romanes' own analysis unjustifiable. Applying this insight requires close specification of the objective criterion which is to justify the inference of course; more important, it requires specification also, in a form independent of the experimenter's subjective experience, of the mental characteristics which are to be inferred. That these characteristics might still be based initially on ones abstracted from subjective experience is of comparatively little moment; the important requirement is that they be specified independently of such experience so that they can in turn be examined and assessed independently⁸.

The proper or available subject matter for comparative psychology is thus, initially at least, mind in its external manifestations, rather than mind in its internal constitution and relations. This may seem to be halfway on the road to behaviourism already, but it is not really. In the first place, this formulation of the domain of comparative psychology does not eliminate, or even significantly attenuate, the reference to mind; it merely explicates the reference. In the second place, and as we shall see, behaviourism was the product of antithesis rather than one of steady development.

Within comparative psychology itself, the lesson about subjectivity and its limits was gradually and steadily assimilated. That it was not accepted all at once was due to the conviction already mentioned, shared by most comparative psychologists, that the description of the probable subjective experience of non-human organisms comprised a major part of the explanatory goal at which they were aiming. The descriptions sought were not required to conform to the analytic conventions of any particular theory or school of introspective psychology, but were still expected to be of the same general sort as were characteristic of introspective psychology at the time. Nevertheless, the equivocality of subjective inferences served to reduce their independent role in the actual business of reconstructing the animal mind and left them to be added at the end; in this way, the subjective inferences were prevented from interfering with the sifting of evidence. Somewhat paradoxically, it was largely because the subjective descriptions of the contents of the animal mind were the goal or end-point of research that they could become, at least, harmless in the course of that research; that is, since they came last, there were relatively few additional conclusions dependent on them.

The practical diminution of the role of subjective inference in comparative psychology is exemplified in the work of Lloyd Morgan. Morgan's Introduction to Comparative Psychology (1894) contained much the same mixture of subjective and objective methods as did Romanes' Animal Intelligence (1882), but for Morgan the relative importance of the two methods was the reverse of what it was for Romanes. Morgan (1894, pp. 36-52) made a profound bow to the necessity and desirability of subjective inference as a complement to objective inference, but then (*ibid.*, pp. 53-59) made several methodological assumptions which jointly served to minimize the independent role of subjective inferences. First he assumed, as did Romanes, that the analogy from human to animal mind deteriorates with increase in the amount of phylogenetic separation between the animal and man. Second, he assumed that the deterioration is not a smooth function of any other objectively ascertainable evolutionary process. Third, and most important, he assumed that the analogy would extend to different capacities or faculties differentially, so that the deterioration of the analogy was not necessarily even monotonic. That is, he assumed that mental evolution is not a unitary process; one species may possess one faculty to a greater extent and another faculty to a lesser extent than another species, and thus some animal species may possess certain psychic faculties to a greater extent than does man (as is obviously the case with specific perceptual-motor abilities). None of these assumptions was claimed necessarily to be correct; rather, they were introduced expressly in order to limit severely any systematic but unchecked use of subjective inference. One was required to assume that the analogy does not hold in a given way in a given case unless objective inferences provide an independent account of the animal's mind such that the questioned application of the analogy could most readily seem to follow. In short, objective inferences were to lead in the characterization of the animal

mind, subjective inferences to follow. Thus, while Morgan was not prepared to abandon the dualism of subjective and objective inferences, he was sufficiently sensitive to the demands of his research that in practice he effectively curtailed the claims which could be made on the basis of the former. His method, as he put it, "is the least anthropomorphic, and therefore the most difficult (*ibid.*, p. 58)."

Morgan posited these methodological assumptions in the context of developing his famous canon of parsimony. Their effect is summarized in the canon: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale (*ibid.*, p. 54)." The canon and the reasoning which leads to it thus have the dual effect of encouraging parsimony in general and of subordinating subjective inference to objective reconstruction⁹. The canon also signals the beginning, at least, of an attempt to externalize the mental characteristics which are to be inferred, that is, to formulate them as faculties or capacities without reference to subjective experience of their operation. It is a bare beginning, but the reference to a "psychological scale" on which the abilities of man and beast alike are to be placed takes them one step away from their founding in subjective experience and reflection. It is not a step which Morgan took with confidence, however, or at least not one which he followed up, so that while he carefully based each of his inferences to psychic faculties or capacities on detailed and systematic observations of animal behaviour, he just as carefully translated each inference into descriptions of the contents of the animal's conscious experience.

In 1896, Morgan gave a course of lectures at Harvard, where Thorndike was a graduate student, and it has been suggested that he influenced Thorndike to take up the problem of experimental research on

animal behaviour (Warden, Jenkins, & Warner, 1935, I, p. 27). The main publication that resulted from Thorndike's researches, his "Animal intelligence" (1898), has been justly acclaimed as a classic for a number of its innovations, including the introduction of a rigorous experimental procedure, concentration on typical rather than on extraordinary performance, etc. On the issue which we have been emphasizing here, that of the relationship between subjective and objective inferences in the description of the animal mind, Thorndike was not entirely consistent. In parts of his experimental analysis, however, he took the practice of reconstruction of the animal mind on the basis of objective criteria much farther than it had been taken before. He asked the question, for instance, what has been going on in an animal's mind while it has been learning to escape from a puzzle-box and acquire food?

The commonly accepted view of the mental fact then present is that the sight of the inside of the box reminds the animal of his previous pleasant experience after escape and of the movements which he made which were immediately followed by and so associated with that escape. It has been taken for granted that if the animal remembered the pleasant experience and remembered the movement, he would make the movement. It has been assumed that the association was an association of ideas; that when one of the ideas was of a movement the animal was capable of making the movement (Thorndike, 1898; 1911 ed., p. 99; *italics in the original*).

In other words, the animal is held capable of abstracting from its experience in the puzzle-box a representation of those experiences, a representation which it is able to use to guide its subsequent actions. Thorndike, by contrast, claimed that the animal is capable of no such abstraction, or at least that no such abstraction guides its behaviour; the basis for his claim was that the animal was unable to initiate the behavioural sequence leading to escape and food by means of a new response.

If a cat is given training trials in a puzzle-box, so that it is made to enter the front door of the box and left to learn the

response which will open the door and give it access to food, it will over a series of trials acquire the escape response more and more perfectly. If the cat is then set outside the box on a test trial, with the door open, it will initiate the trial by entering the box of its own accord. If, however, a cat is dropped into the box from the top, and learns the escape response to the same criterion, on a test trial it will not enter the box through the front door of its own accord. Thorndike concluded on this basis that the action of walking into the box is indispensable to learning the sequence: enter the box--escape from the box--eat. Ideas cannot be considered to function in any way independent of such specific behaviour, at least in cats, because the cats which were dropped into the puzzle-box from the top

had exactly the same opportunity of connecting the idea of being in the box with the subsequent pleasure. Either a cat cannot connect ideas, representations, at all, or she has not the power of progressing from the thought of being in to the act of going in (*ibid.*, p. 102).

Thus, whatever cognitions may be said to function in the cat's behaviour are inseparable from the particular responses which the cat has acquired in a given situation.

We may note in passing that the evidence in favour of Thorndike's claim is not conclusive. In particular, it depends upon the assumption, which Thorndike explicitly made (*ibid.*, p. 29), that the associations involved in learning to enter and escape from the puzzle-box are representative or typical of the kinds of associations which the animal is required to make in its day to day life. With this qualification, which is an important one, the most significant feature of Thorndike's analysis is that he was able to characterize in unprecedented detail the mental operations of cats by analysing

the divergent actions which different classes of mental operations would be capable of initiating. He thus carried the process of objective inference of mental operations and capacities to a new level of precision. The 'either-or' form of Thorndike's inference as quoted exemplifies the kind of reconstruction of mental operations that is possible. Either one set of mental operations, or a functionally equivalent set, has taken place; either the cat cannot abstract the reinforcement contingencies from its exposure to the experimental situation, or it cannot utilize this abstraction in the determination of its subsequent behaviour.

On the other hand, Thorndike was quite determined that experimental investigations of the skills, capacities, and general behaviour of animals should not only "give the much-needed information how they do it, but also inform us what they feel while they act (ibid., p. 26; italics in the original)." Thus, after analysing the mental operations and capacities of cats in the way just described, he went on to consider the quality of the cat's consciousness in the course of the action.

It is most like what we feel when consciousness contains little thought about anything, when we feel the sense-impressions in their first intention, so to speak, when we feel our own body, and the impulses we give to it. Sometimes one gets this animal consciousness while in swimming, for example. One feels the water, the sky, the birds above, but with no thoughts about them or memories of how they looked at other times, or aesthetic judgments about their beauty; one feels no ideas about what movements he will make, but feels himself make them, feels his body throughout. Self-consciousness dies away. Social consciousness dies away. The meanings, and values, and connections of things die away. One feels sense-impressions, has impulses, feels the movements he makes; that is all (ibid., p. 123).

Thus, for Thorndike, as for Morgan, detailed analysis of an animal's mental operations were followed by descriptions of the animal's

subjective experience, such descriptions being based on a dubiously effective empathy or equally dubious analogy between human and non-human experience. At the risk of labouring the point, the difference between the two sets of inferences is, again, not one of 'inner' and 'outer'; the operations of mind are at least as 'inner' and unobservable as are the impressions received by that mind. The difference is rather that the former inferences make contact with observable behaviour through specification of the actions which the mental operations (or equivalent ones) lead to, while the latter do not. The latter have observable causes, but the former have observable effects. The differences may be highlighted by the consideration that the former set of inferences can be made as confidently if the subjects are ants or Martians as they can if the subjects are cats or dogs--subject to the qualification mentioned above, which applies to all four species equally--while the latter quite clearly cannot. For the latter inferences, we must already have some knowledge or conviction concerning the quality of the experiences of the subject organism, a conviction which for Thorndike as for Morgan and Romanes amounted to a predisposition to accept the analogy from human minds to others; but for the former inferences, no such prior knowledge or predisposition is necessary.

Nevertheless, Thorndike carried still further than Morgan the practice of effectively isolating the subjective inferences from the objective ones. Morgan placed limitations on the use of subjective inferences which in effect subordinated them to, and prevented them from interfering with, objective inferences. Thorndike in addition treated the two kinds of inference as separate and only minimally related problems, by virtue of his conclusion that subjective experiences were not effectively implicated in the determination of action.

Finally, and to complete the series, we may mention the work of Leonard Hobhouse. Hobhouse's Mind in Evolution (1901) carried the conceptual and methodological development of comparative psychology as traced here to its highest level to that time. Hobhouse formulated his own version of the canon of parsimony, one that was closer to Occam's original razor: "In comparative psychology the legal maxim must hold, that the thing which does not at some point or other appear in action must be treated as non-existent (Hobhouse, 1901, p. 54)." Following on this formulation, he made the clearest distinction to that date between the 'mind' and the 'consciousness' of animals, and with some regrets took the final step in eliminating the experimenter's subjectivity as an analytical tool--and hence, in effect, in eliminating references to the subjective experience or conscious contents of the animals being tested--from comparative psychology.

I am not here concerned so much with the kind of consciousness that animals may enjoy, as with the bearings of their experiences on their actions and achievements. In describing the behaviour of an animal, to use terms derived from the human consciousness is often the only way of avoiding intolerable prolixity. Properly guarded and corrected by attention to points of difference as well as resemblance, such usage can lead to so little error that, even if we were ultimately to decide that all animals were automata, no change but that of names would be needed in our account. By "feeling" in an animal, then, we shall mean a state essentially similar in causation and function to that which we know as feeling in ourselves. Whether it is similar in other respects, is a question which we do not decide by merely using the term. And so with similar terms (*ibid.*, p. 90).

In addition to such conceptual refinements, Hobhouse made numerous methodological and substantive advances in the experimental study of animal behaviour. He effectively criticized and corrected the artificiality of Thorndike's puzzle-box studies and conducted his own experiments in situations much closer to the animal's day to day life situations. He prefigured in great detail Köhler's studies of

insightful behaviour in apes, made highly sophisticated studies of what was later to be called 'perceptual learning' in cats, dogs, and monkeys, and incorporated his findings into an evolutionary theoretical structure that was in equal parts parsimonious and comprehensive. His work cannot, however, be afforded more detailed treatment in considering the development of comparative psychology because, rightly or wrongly, it was almost totally neglected and had practically no lasting influence¹⁰. It is at least possible that comparative psychology might have fared better if it had.

In summary, comparative psychology in the first two decades of its existence made numerous advances in investigative methodology and in the scope and precision of its findings. Perhaps even more important, it worked also at achieving a gradually refined, even if usually only implicit, conception of its subject matter, as consisting in the adaptive character and capacities of mind as revealed through its effects in promoting or potentiating the efficient behavioural adjustments of organisms to their environments. Through all of this, its goal remained, with remarkable constancy, that of reconstructing the pattern of evolutionary development of mind or of capacities for increasingly complex adaptive behaviour. In Romanes' terms, the goal was "that of tracing, in as scientific a manner as possible, the probable history of Mental Evolution, and therefore, of course, of enquiring into the causes which have determined it (Romanes, 1884; 1969 ed., pp. 11-12)." For Morgan, it was "to ascertain the limits of animal psychology (Morgan, 1894, p. 53)," "to discuss the relation of the psychology of man to that of the higher animals (ibid., p. ix)," and "to indicate the relation of mental evolution to evolution in general (ibid., p. 358)." For Thorndike, "The main purpose of the study of the animal mind is to learn the development of mental life down

through the phylum, to trace in particular the origin of human faculty (Thorndike, 1898; 1911 ed., p. 22)." Finally, for Hobhouse, the purpose was "to trace the main stages of orthogenic evolution, which we have provisionally identified with the evolution of Mind (Hobhouse, 1901, p. 9)." (Orthogenic evolution is evolution toward a higher type.) These four writers comprise a convenient group for illustrating the development of the science, but similar formulations differing only with regard to the role assigned to subjective inferences and hence conscious experience could be taken from the writings of Jennings, Loeb, Lubbock, and others. The tracing of the conceptual development of the science will take on added significance in the context of the two following sections, where it will be seen to indicate a direction that comparative psychology, after becoming embroiled in further conceptual and methodological difficulties, could possibly have taken but did not; and will also serve as a source of contrast by which to highlight these difficulties.

II. Comparative Psychology and Functionalism.

In the attempts at the reconstruction of the animal mind as detailed so far there is a possible pitfall. From behaviour we can infer the operations and general functionings of mind. From the analogy between human and animal minds we can infer--or it was taken that we could infer--the subjective experiences or conscious contents that accompanied or were the inner aspects of such operations. What could seem more natural, therefore, than to explain the observed behaviour as a function of these conscious contents, or in other words, to show how the subjective experience of the animal functioned in determining its behaviour? Such a procedure would be in line with our common sense expectations concerning the relationship of thought and experience to

action, and given that the analogy was to be used at all, there would seem to be little reason not to use it in this way.

The reason for not using it in this way of course is that the analogy is, as we have seen, incapable of systematic justification or critical modification. Its use is not warranted at all, and the only saving grace attached to its use was that it was coming to be used less and less centrally as comparative psychology developed. Refraining from use of the analogy would not constitute a repudiation of the subjective experience of other organisms, but merely the recognition that use of the analogy could not in any case encompass such subjective experience. That is, the particular subjective experiences of an animal being tested could not really be inferred on the basis of the analogy, but merely posited, since the analogy amounts to a statement that subjective experiences of a certain sort--similar to those of man--occur. In this respect, use of the analogy can only echo already-held beliefs, derived from whatever source, about the subjectivity of animals. Attempts based on use of the analogy to show how an animal's subjective experience determines its behaviour would therefore involve the use of an incorrigibly artificial construct in a central role, would amount to explaining the known by, not merely the unknown, but the unknowable.

None of this, however, was fully appreciated at the time. The only writer, among those mentioned, who did not rely on the analogy was Hobhouse, and he did not repudiate it so much as abandon it; in any case, his lead was not followed subsequently. None of the other three writers discussed above entirely avoided the pitfall of explaining behaviour with reference to subjective experience, but for different reasons they did not become altogether ensnared in it. For Romanes and Morgan, as already indicated, the description of possible or probable subjective experience

was an end point in the reconstruction of the animal mind; as a result, such descriptions did not have a major systematic function in any further chain of reasoning. Morgan, in addition, placed such stringent limitations on the use of subjective inference that it was effectively, even if not theoretically, subordinated to objective inference.

Thorndike treated the two kinds of inference as separate and only minimally related tasks, being convinced that in point of fact subjective experiences did not function separate from behaviour in guiding subsequent behaviour. For these writers, subjective inferences were more or less isolated from the rest of the task of reconstruction. Being isolated, such inferences were, if of little use (from our standpoint), at least relatively harmless. By contrast, in most of the behaviour research associated with the American functionalist movement in psychology, there were no such factors operating to minimize or mitigate the role of subjective inferences and the consequences of their use.

Functionalism.

The genesis and development of functionalism in American psychology has been amply described by Boring (1950a) and related by him to such self-consciously American turn-of-the-century preoccupations as individualism, practicality, democracy, and the loss of the open western frontier (see also Boring, 1950b). Functionalism was in large part based on or inspired by evolutionary theory of course, just as was comparative psychology in Britain, but with a difference. Functionalism, to begin with, was a general movement in or orientation toward psychology (it was never a system), of which comparative psychology was one small part. The general character of the difference between British and American post-Darwinian psychology is sometimes summarized by saying that in British psychology the dominant focus of interest was in explicating how we all--

men and animals--arrived at our present positions, with the emphasis both in comparative and in differential psychology on these positions as already established; while in American functionalist psychology the focus of interest was more on explicating and harnessing the principles of development and change, with the emphasis on the ongoing process of change itself. This interpretation of the difference in the two psychological traditions, as reflecting general differences in national mood or preoccupation, is, as an interpretation, admittedly facile. Furthermore, it requires serious qualification, inasmuch as the most virulent forms of 'social Darwinism'--which might perhaps better be called 'social Spencerism'--arose also in the United States, as justification for the current distribution of wealth and power (Corwin, 1950). Still, whatever the limitations of the general account, it is quite true at the level of simple description that a liberal, progressivist, and optimistic version of evolutionary dynamics and their implications underlay and provided the thematic background for much of functionalist psychology.

It followed, therefore, that the central concern in functionalist psychology was with the processes of adaptation, growth, and development--in the individual, the species, the phylum, and the animal kingdom as a whole, in roughly that order of descending emphasis. The way in which this concern was initially expressed marks another source of the difference between American and British psychology. At first, psychology in the United States, especially the naturalistic and experimentally inclined psychology associated with the universities, was derived in large part from German influences, and particularly the introspective experimental psychology of Wundt. This 'new psychology', as it was called, was of course a psychology of the fundamental contents

of consciousness. The way in which the American interest in developmental processes and individual differences, in conjunction with the interest in the implications of evolutionary theory, gradually transformed this somewhat abstract conception of psychology into the more concrete functionalist one is described in detail by Woodworth (1931). The transformation itself was remarkably simple and straightforward; indeed, what is largely of interest in it is that it was gradual, non-revolutionary, cumulative, and quite fully recognized at the time. That it was gradual and non-revolutionary is indicated clearly enough by the piecemeal way in which it occurred and by the relative lack of opposition which it encountered; its main opponent was Titchener, who did not repudiate it so much as merely suggest that it was occurring too soon. That it was quite fully recognized and even intentional on the part of its chief agents is indicated by the early statement of James (1884) and the later one of Titchener (1898), both contrasting psychologies of structure with psychologies of function, or as Titchener put it, the "is" with the "is for".

Functionalist psychology was thus neither a continuation nor a reaction, but an adaptation of the Wundtian, introspectionist, structural psychology to the particular demands of the American situation. British psychology, by contrast, was from the beginning less inclined toward analytic introspection and experimentation than were either the German or the American sort. While British psychology was introspectionist, it was not systematically so, and did not have such an overriding concern at any time with the specific, analytically determined contents of consciousness. In consequence, the hold of analytic introspection, and hence of consciousness--the specific kind of consciousness which could be discovered through analytic introspection--as the basic subject

matter of psychology, was much stronger in Germany and the United States than in Britain¹¹; it was strong enough in the United States that in the course of the quite non-revolutionary passage from Wundtian to functionalist psychology both introspection as a collection of methods and the associated consciousness as the subject matter of psychology were willingly retained.

The consciousness that was retained was, nevertheless, regarded in a somewhat different way than Wundt (or Titchener) regarded it. The emphasis in functionalist psychology was, quite reasonably, on the uses of consciousness, what it was good for, how it facilitated the adaptations of organisms to their environments. James (1890) wrote at length on the presumable efficacy of consciousness as a regulator of the complex, finely tuned, and inherently unstable neural mechanisms of the highly evolved human brain¹². Angell explicitly based his psychological writings on the principle that "consciousness is an organic function whose intrinsic occupation consists in furthering the adaptive responses of the organism to its life conditions (Angell, 1904, p. 79)." Functionalism, he stated in brief, is the "psychology of the fundamental utilities of consciousness (Angell, 1907, p. 70)."

Neither James nor Angell, nor, for some time, almost anybody else, made either a methodological or a theoretical distinction between consciousness as a form of differential and directed sensitivity to different aspects of the environment, and consciousness as a state of basic personal awareness consisting of specific private experiences. Nor, for their purposes, was there any particular reason to do so, although the distinction is, as we have seen (p. 152), an essential one if we are to talk about the consciousness of animals. The significance of the two aspects of consciousness and at least some aspects of

the distinction between them were certainly acknowledged, as when Angell spoke of attention as "the very heart of consciousness, its most important centre of vitality (1904, p. 64)," and, on the other hand, as when both writers took part in the acrimonious debates concerning whether, and how, consciousness as a state of basic awareness could have evolved from some previous state which contained no traces of it. But while the distinction was made, what was distinguished was regarded as no more than different points of view in the consideration of consciousness--as indeed, in one sense, it was. That is, the distinction that was made was the perspectival distinction, fundamental to functionalism, between what consciousness is and what it does, between its form and its functions, its contents and its operations, its "is" and its "is for". It was therefore, in light of the gradual way in which the functionalist orientation developed, a distinction that could support a difference in emphasis, but not a difference in basic theoretical and methodological stance. While it was the latter aspects of consciousness, that is, its functions or operations, that were of principal interest to the functionalists, the consciousness that was thereby studied in terms of its utility was clearly understood to be the same consciousness that could otherwise be studied in terms of its form, internal properties, or content. That is, the functionalists were concerned with the adaptive significance of that same consciousness of which Wundt and Titchener were investigating the structure¹³. The distinction between the form or contents of consciousness on the one hand and its functions on the other in no way corresponded to a distinction between a consciousness that could be studied and a consciousness that (at least in animals) could not, between consciousness as publically inferrable and consciousness as inescapably private. It would in fact have been virtually impossible

for the two distinctions to correspond; it is of principal importance for them to do so only in animal psychology, which was not initially at the centre of the functionalists' concern, and even within animal psychology use of the analogy was widely regarded as a means whereby the second distinction (between investigable and non-investigable consciousness in animals) could be circumvented. Indeed, it would have seemed bizarre to any psychologists of the day to suggest that the two distinctions should correspond; consciousness, after all, is what it is, whatever it is, and while it can be studied in different ways, it is surely the same consciousness that is being studied.

Functionalist Comparative Psychology.

Thus, functionalism was concerned with the adaptive significance or utility of that consciousness which exists as a state of awareness independent of its functions, with the adaptive significance, that is, of those conscious contents that include what was designated previously as 'subjective experience' or 'experience as such'. Given the increased liberalism in contemporary psychology concerning studies of consciousness, it is possible to look with more sympathy on this type of investigation than might have been possible, say, twenty years ago. Indeed, so far as the enterprise was applied to human beings, who are often capable of fairly accurate introspection, it may be worthy of considerable respect. But for comparative psychology, where there was no opportunity for independent reporting of conscious contents and of behaviour, the consequences of functionalism's stand were immediate and disastrous. The general programme and orientation of functionalism made it inevitable that comparative psychology would fall into the pitfall described at the beginning of this section, that of using inferred subjective experiences as a systematic basis for accounting for an

animal's behaviour, and would fall into it, furthermore, in a particularly debilitating manner. Let us see why this should be so.

The subject matter of functionalist psychology, human and animal alike, was consciousness as such, considered in terms of its functions and adaptive significance. That is, it was the adaptive significance of the specific and particular subjective experiences or mental operations which an experimental subject was having or performing while in the experimental situation; and in the case of animal subjects, of course, these various conscious contents had to be inferred rather than reported. Such particular conscious contents--subjective experiences and mental operations--clearly had functions, and it was these that were of interest, but because of their particularity could not have functional equivalents. That is, they could not be approached or investigated through a consideration of their functions or effects but had to be considered directly, in the way in which they actually occurred. As detailed in the previous section, it is only when functionally equivalent sets of inferences can be drawn that it is possible to make any sort of testable reconstruction of an animal's mental life; and in functionalist comparative psychology this condition was lacking. Consciousness or subjective experience or mental operations as such--anything as such--have, again, no functional equivalent. There is no functional equivalent for the existence or the purely existential characteristics of anything, even when, as in the present case, the reason for directing attention to that thing is to ascertain and analyse what functions it in fact has. Since it was the function of consciousness as such, or of the specific contents of consciousness, that interested the functionalists, it was therefore of central importance to the programme of functionalist comparative psychology to describe

the particular content of an animal's consciousness, just as they and the structuralists alike described the particular content of human consciousness, so that the specific functioning of this consciousness in determining the animal's behaviour could be demonstrated.

Boring (1950a) neatly summed up the three-stage method which was typical of functionalist comparative psychology from the late 1890s until about 1915, in a bald statement of the relationship between observation, subjective inference, and reconstruction of the animal mind based on such inference:

The rule of functional animal psychology of that date was that, when you have finished your observations of behavior, you use the results to infer the nature of the animal's consciousness and then show how those processes function in the animal's behavior (Boring, 1950a, p. 556).

Where this rule departed from the method of Romanes and his successors was simply in the addition of the systematic requirement that the inferred conscious "processes" must be shown, and shown with specific detail, to "function in the animal's behavior". The functionalists, however, possessed no new and better ways of gaining access to these conscious processes or subjective experiences of non-human organisms, and so had to rely more than ever on the assumed adequacy of the analogy from human to animal minds.

In thus employing the analogy between human and animal minds as the tool for the interpretation of the latter, the functionalists, as we shall see, constructed a comparative psychology that was significantly different in emphasis from the contemporaneous British movement in the field (to which we have effectively assimilated Thorndike¹⁴). Both movements, of course, made much use of the analogy. What differentiated functionalist comparative psychology was first, its emphasis on the functional utility or significance of specific conscious

contents (as just discussed), and second, its character of being more closely derivative of introspective psychology. These two features jointly prevented functionalist comparative psychology from making those compromises with the exigencies of research which established the British programme of work in the field as viable despite its incorporation of the analogy in a central role. Instead, and in conjunction with the generally accepted constraints on psychological theorizing, these two features led the movement, quite against the wishes of many of its ablest exponents, toward an almost trivially sensationalistic, 'passive organism' model of the mental life of animals. In the process, the field of comparative psychology could hardly help taking on the shallow mentalism and estrangement of data from theory against which behaviourism was advanced as an explicit reaction.

It might be suggested that the development in functionalist comparative psychology of a sensationalistic, passive organism model of mental life could follow in part from the continuing influence of the British empiricist tradition, in which the passive organism model had been gradually developed and fully adumbrated from Locke through the Mills. Certainly the continuing influence of the British empiricist and associationist model was invoked many years later by Hull (1943) as providing the thematic background for the psychology of his own day. But while the influence of this tradition may indeed have been implicated in the course of development of functionalism, it could hardly provide a sufficient account; the British comparative psychologists discussed previously were, presumably, equally subject to the influences of British empiricism, and yet they managed at least in part to go beyond a passive organism model in considering what animals were

capable of doing.

The influence of introspective experimental psychology--which was itself of course largely a German formalization of the tenets of British empiricism--was no doubt directly implicated to some degree in the acceptance of a sensationalistic model within functionalism. Wundt's and Titchener's psychological systems were themselves, by the late 1890s, becoming increasingly sensationalistic (especially Titchener's), and had always been elementaristic. The status of functionalist psychology as being partly derivative of introspective psychology afforded a high initial credibility to descriptions of the consciousness of animals based on the same terminology and structural organization typical of the more straightforward experiments in human introspection. The difference between American and British comparative psychology was, it is quite true, not fundamental in this respect, since in both cases the descriptions of animal consciousness were based on the model of human introspection. In Britain, however, the descriptions were relatively imprecise, often based on the somewhat vague classifications of Locke or Hume (cf. footnote 3 to this chapter); in the United States, by contrast, they were considerably more precise (or more precise sounding) and more explicitly sensationalistic, as befitted their more immediate origin in experimental introspection¹⁵.

The principal influence of introspective psychology, however, was an indirect one, manifest through its original influence on the constitution of functionalism, whereby functionalism was to be concerned with the utility of the particularized individual consciousness. In brief, the incorporation of a sensationalistic, passive organism model in comparative psychology, while no doubt facilitated by the other influences mentioned, followed principally from the methodological

difficulties in taking the functions of consciousness qua state of awareness as the object of investigation. To clarify these difficulties, let us again consider the tension that would be present in any account that attempted to incorporate both objective and subjective inferences. This tension did not severely affect functionalist comparative psychology, because the two forms of inference were not generally combined. Nevertheless, the way in which the tension was avoided for functionalism makes an illuminating contrast with the way in which it was resolved by earlier writers.

The tension, of course, is that between the two available bases for inferring the existence and operations of mind or of mind-like activity, that is, between objective inferences based on observations of behaviour and subjective inferences based on the analogy from human to animal minds. Again, from the observations of behaviour in conjunction with a carefully specified criterion we can infer the kinds of integrative or adaptive capacities which an animal manifests in its behaviour, and from the application of the analogy to such observations we can supposedly infer the actual mental operations and experiences which are present to the animal's consciousness. If these two sets of inferences yield different conclusions, there is a problem in reconciling them; if they do not, it becomes questionable whether both are necessary. The tension was resolved or at least mitigated for the writers previously discussed by their considering the two forms of inference as relating to more or less separate questions; for Morgan, additionally, by his making subjective inferences effectively subordinate to objective ones; for Thorndike, additionally, by his conclusion that subjective experiences play little or no part in the determination of behaviour. For the writers associated with functionalist comparative

psychology, however, the tension could not be resolved or avoided in quite this way. Subjective inferences could hardly be subordinated to objective inferences inasmuch as it was the relationship of private or subjective experience--the natural goal of subjective inferences--to ongoing behaviour that was the focus of interest. At the same time, objective inferences could not be wholly subordinated to subjective inferences because of the realization and general acceptance that all inferences had in some way to be uniquely determined by observations of behaviour if they were to amount to anything more than rank speculation.

Nevertheless, objective inferences were, in a way, subordinated to subjective inferences, or in another way assimilated to them. Some such subordination or assimilation was presumably inevitable. The most effective product of objective inferences was mental capacities or faculties for adaptation, conceived independently of any conscious experience; and such capacities could be of little relevance or interest in the context of a primary and irreducible concern with the functional utility of specific conscious experience. What was required, therefore, was some way to combine the particularized content of subjective inferences with the public observational basis of objective inferences, some way, in other words, to place subjective inferences--inferences to subjective experiences, predicated on the analogy from human to animal minds--under the strict control of objective criteria.

There was, furthermore, no apparent difficulty in restricting the latitude of subjective inferences in just this way. Such restrictions were, after all, continually being increased as part of the methodological development of comparative psychology. The functionalist comparative psychologists were heir to all the constraints on the drawing of subjective inferences which Morgan had introduced and summarized in his canon, as well as to the methodological strictures in favour

of rigorous experimental procedures advanced by Thorndike. Morgan's canon of parsimony required that all subjective inferences (and objective ones as well) be the minimal ones available. The methodological strictures associated with the development of experimental methods required that the factors relevant to the performance of a task required of an animal be all, as far as possible, explicitly specified and hence controlled in the establishment of the experimental situation. The subjective inferences, that is, were to relate as uniquely and precisely as possible to the animal's experience of and reactions to the specifiable parameters of the stimulus situation.

In the context in which they were originally introduced, these restrictions did not have quite the same intent or effect as they came to have in functionalist comparative psychology. Morgan and Thorndike employed both subjective and objective inferences, and the function of the restrictions on the drawing of inferences was in large part that of regulating the relationships between the two kinds. The restrictions served to emphasize objective inferences based on observable behaviour, to minimize the latitude of subjective inferences, and consequently to subordinate subjective inferences to objective ones. Subjective inferences were subordinated, that is, both by being required to conform to the initial interpretations based on objective inferences and, inasmuch as they were the end point in the inferential chain, by having little or no specific functional significance assigned to them. As a result, the burden of explanation for the most part fell on the objective inferences.

In functionalist comparative psychology, by contrast, the restrictions could serve only to reduce the latitude of subjective inferences. They could not regulate the relationship between subjective

and objective inferences since, due to the emphasis on specific conscious experience as the subject matter of psychological investigations, objective inferences were hardly implicated at all. It is of the greatest importance to emphasize, furthermore, that subjective inferences could only be restricted by these means; they could not be refined, in the sense of making increasingly precise or accurate contact with the animal's subjectivity, because of the insuperable difficulties described previously in gaining access to such subjectivity and in checking the worth of the inferences to it. In the British work in the field, it is impossible to judge whether or not subjective inferences were becoming refined or more accurate, but it is certain that, in addition to being restricted by the methodological constraints, they were being guided and directed by the account of the animal mind already established on the basis of objective inferences. They were closely determined by the behavioural observations, that is, in two ways; they were restricted by the methodological constraints, and directed by the objective inferences. In functionalist comparative psychology, since there were practically no objective inferences, the subjective ones were merely restricted by the methodological constraints.

Since such methodological constraints on their use comprised the only check on the drawing of subjective inferences in American research, the increasingly sophisticated employment of these constraints became implicated more and more centrally in the development of comparative psychology, as a means--the only means--for making the inferential accounts precise and unambiguous. But again, while such constraints could make the inferences progressively more 'objective', in the sense of being based on publically observable events and publically applicable criteria (i.e., so that different observers could

make the same inference), they did not make them any more accurate. Objectivity and accuracy, in this instance, were quite separate and unrelated considerations, even if they were, naturally, not recognized as such. Objectivity was a consideration relating to the information from which the inferences proceeded; accuracy was one relating to the consciousness to which they proceeded; and all the procedures designed to maximize the former could do nothing to increase the latter.

Thus, the interpretation of experiments carried out within the programme of functionalist comparative psychology came to display a curious pattern. On the one hand the methodological strictures served to minimize the choice and extent of subjective inferences, and did so more and more as the methodological sophistication of animal behaviour studies continued to increase. As a result, the inferred conscious contents gradually became correspondingly restricted and impoverished as to their constituents. On the other hand, because of the priorities of functionalist psychology these impoverished inferred conscious contents were accorded enormous functional significance. To put it another way, the strictures on the drawing of subjective inferences--the canon of parsimony, the increase in experimental control--refined these inferences in the only way in which they could be refined and rendered uniquely determined by the experimental situation; they refined the inferences, that is, by rendering them little more than a reflection, or a translation into the sensationalistic language of introspective psychology that was held appropriate for the description of subjective experience, of the animal's behaviour and of the parameters of the experimental situation as they were judged to impinge on the animal's sensory apparatus. Looking backward from our present perspective, we can see that the reason for such an impoverishment of

subjective inferences as a function of increase in experimental control--the same experimental control that was supposed to render them precise and unspeculative--was simply the incorrigible artificiality and unwarrantability of such inferences. As the latitude for speculation decreased, so did the latitude for subjective inferences; but the subjective inferences could only be restricted rather than sharpened. As a result, the more they were restricted, the closer they came to being merely trivial. But such almost trivially inferred states were supposed to comprise conscious contents, which were supposed to have functional utility; they were what was held to guide behaviour, and explicating how they did so was taken to be the most significant part of any comparative psychological experiment. In short, at the same time that subjective inferences were becoming so restricted and hemmed in that their artificiality and triviality could hardly fail to become evident, their importance in revealing the determinants of behaviour was being magnified as never before.

As a result, a sensationalistic, passive organism model of mental functioning could hardly be avoided. The alternative to a passive organism model of mental activity is one in which the organism performs internal operations of a sort which do not simply mirror its surroundings and its internal biological conditions. Such an 'active organism' model is certainly compatible with an evolutionary emphasis on the development and functioning of adaptive processes, and was indeed the theoretical goal of much of functionalist psychology. However, the elaboration of such an active organism model would require some sort of specification or description of the internal activity. The only effective way to make contact with such activity is through objective inferences from the organism's behavioural capacity, because

only such inferences relate to internal capacities and operations that can be specified in terms of their functional significance. Functionalist psychology had little place for such descriptions of capacities or functionally equivalent sets of internal operations however, because of its particular concern with individual consciousness. It was thus limited for the most part to subjective inferences. Subjective inferences, in turn, might conceivably have been used for the description of the particular mental operations that an animal was performing. Such inferences could not have been very effective, it is true, since subjective inferences, necessarily based on the use of the analogy, never are. But they might at least have stimulated the development of objective methods--methods, that is, based on the use of objective inference--for the description and analysis of such mental operations, if the operations themselves had been agreed to be of principal interest. This suggestion is purely conjectural, admittedly, but it is strengthened by the fact that the replacement of subjective methods with objective ones for making the kinds of descriptions generally desired, that is, those in which the actions of the experimental animal are regarded as reflections of its surroundings and its internal physical states, was much the course that psychology followed in giving birth to behaviourism.

But this is getting ahead of the story. Within the programme of functionalist comparative psychology, subjective inferences--inferences to the private mental lives of individual organisms--could not legitimately be directed to the description of mental operations, regardless of how heuristically valuable such inferences might have been. In the absence of any account based on objective inferences (which could serve to subordinate the subjective ones), subjective inferences could

be prevented from lapsing into uncontrolled speculation only, as we have seen, by being kept rigorously related to the specifiable parameters of the experimental situation. Mental operations of the sort which would characterize an active organism model could not, almost by definition, be so restricted; such operations were hence outside the range of control which could be exercised over subjective inferences, and such inferences were, accordingly, restricted to the minimal sensationalistic ones.

The passive organism model of mental life which became typical in functionalist comparative psychology was thus in large part a methodological artifact, resulting from the constraints imposed on subjective inferences in an attempt to make them objective, rather than simply a product of the continuing influence of the empiricist tradition or of any other longstanding philosophical trend. It is not less important in the subsequent history of psychology on that account, but its derivative status highlights the more immediate significance of the methodological constraints, and begins to indicate how the model could be retained, as a result of similar methodological considerations, in behaviourism. Furthermore, the attempt by the functionalists to make objective descriptions of subjective states could not, it is apparent, be ultimately successful; it resulted only in the organism's growing ever more passive. The 'organism' thus studied in terms of its subjective experiences became more and more a mere reflection or sensationalistic translation of the stimuli which it received and of the responses which it emitted. Between stimulus and response there figured little more than a mental representation of the stimulus and a mental representation of (or instinctive impulse to) the response¹⁶.

A Representative Experiment.

It is potentially misleading to describe the emergence and trivialization of the passive organism model entirely as a gradual process of development, for the artificial quality of the theoretical accounts advanced within functionalist comparative psychology was more or less characteristic of the field from the start, and increased only slightly throughout the short life of the movement. It resulted, as has been indicated, not from the progressive elaboration of a set of workable although flawed principles, but from the thematic influences that were present in almost fully developed form from the beginning. The interplay of observations of behaviour and inferences to subjective experience in the interpretation of the animal mind is brought out forcefully in as early a study as Small's investigations of learning in rats (Small, 1899, 1901). Small's study included the experiments in which he introduced the maze as a tool in the study of animal learning. In the experiment to be quoted here (in which the discussion is unusually concise), a rat was trained to gain access to a portable goal box containing food by tearing off a paper seal which obstructed the door to the box. On the test trial, the door to the box was further secured so that it required a firm push to open it after the seal had been removed. At the beginning of the quotation Small discussed the quality and contents of the rat's consciousness before it encountered the new obstruction.

The train already formed may be figured somewhat as follows: feeling of hunger, sight of box, smell of food (these two probably simultaneous), curiosity, location of food in box by smell (and sight), tearing off paper, getting food, pleasurable state. In some instances, as has been noted in considering the preceding groups, the first term of this hypothetical series drops out, and the mere sight of the box is sufficient to start off the train. (It is quite possible that the instinctive acquisitiveness furnishes the organic basis for the series in such cases. It is highly unlikely that any excitation of a purely sensational character would furnish the motive force.) The connection

of these links becomes so intimate that when the rat is normally hungry the appropriate movements are gone through with immediately upon seeing the box introduced into the cage. Now, when this associative process is broken up at the biting-off-paper point, as in this experiment, what happens in the rat's mind? The manifest purpose of the animal is to get inside the box, and this desire to get inside is coupled with the idea of getting in through the door. The modified form of the association train may now be: hunger, a mixed image, motor and visual, of entering the box through the door, getting the food, pleasure. That is, one of the terms of the chain is variable--the association is not determinate. When this term is expunged, another one, perhaps a suppressed one, rises to take its place. Of course it is not necessary to postulate such a process as the following in the rat's mind: "Biting off paper fails of its usual result, therefore I'll try another method." The only necessary elements are: the persistence of the feeling of hunger, the location of the food inside the box, either as a present smell-sensation or as a memory of getting the food inside the box, or both, and the memory of getting in at that place. This last accounts for the constant return to, and the poking of, the door, but, as she is not hurt by it, her boldness increases; and this being further stimulated by the smell of food, finally impels her to force open the door.

The pausing of the rat when the door unexpectedly failed to open might seem to imply reflection; but this is not so in any strict usage of the term reflection. Surprise and disappointment would be quite sufficient to restrain activity for the time; and these affections would preclude the possibility of reflection unless reflection is used merely in a descriptive sense to designate the transition from this passive state to an active state under the re-surging impulse of hunger. That the rat feels "why" or "what" is certain, that she thinks "why" or "what" is both doubtful and unnecessary...

It is also clear, I think, that what properly may be called ideas, find slight place in the associative process. Crass images--visual, olfactory, motor--organic conditions, and instinctive activities are assuredly the main elements. That these elements may bleach out and attenuate into ideas is not impossible. Analogy with human experience would indeed point to that conclusion (Small, 1899, pp. 153-155).

Small's interpretations of his experimental observations were typical of many that were made during this period, by Kinnaman (1902), Kline (1899), Watson (1903, 1907), and others. The general rule is that stimuli lead to sensations, which lead to associations, which in conjunction with instincts lead to behaviour. There were of course theoretical debates going on, but they tended to centre on

the relative importance of sensations versus instincts in the mental life of animals, or on the differential role of stimulus-produced sensations versus organic ones in directing behaviour (Watson's 1907 paper, which he presented as a more precise test and confirmation of Small's hypotheses, was addressed to this latter question). The inferred train of associations was typically similar to that brought out in Small's discussion, containing sensational, impulsive, and motoric (response) elements indifferently; it was almost classic associationism, or a sort that, one can presume, would have been quite acceptable to the Mills or Hartley, and certainly to Wundt¹⁷. The point to be emphasized is that in practically all such work the mental aspect of the animal's activity was conceived almost entirely in terms of what it has, rather than in terms of what it does. In Small's paper there were almost no inferences to capacities or mental operations, of the sort which characterized Thorndike's report a year earlier. Both writers agreed in concluding that 'ideas' play little part in the determination of the animal's behaviour. But for Thorndike this conclusion was reached by considering what responses could be potentiated by the presence and functioning of ideas, responses which his experimental animals did not perform; for Small the conclusion was dependent only on the possibility of postulating a sufficient number of 'impulses' --drives or instincts--that could interact with the animal's experience to stimulate whatever behaviour was observed to occur. The chain of inferences in Small's discussion started with the rat's observable behaviour and the characteristics of the experimental situation; progressed to the rat's sensations and feelings, which latter were either those arising from postulated instincts (e.g., curiosity) or those resulting from experimental manipulation (e.g., of deprivation

schedules); and from these sensations and feelings in conjunction with behavioural instincts moved back to behaviour again. The inferential chain, in brief, proceeded from stimuli (including movement-produced stimuli), to the organism, to responses; but the 'organism' component of the inferential chain was almost entirely passive, consisting of sensations, instincts, feelings, and impulsions. The inferred mentation, that is, was a passive process; what the rat did was a function solely of what it subjectively experienced and of the, not explicitly specified, laws of association. In every instance where the possibility arose that the rat was initiating a train of mental operations (reflection, analysis of relationship between action of tearing off seal and expected consequence), Small concluded solely on the basis of parsimony that it was not doing so. His conclusion was spuriously facilitated by his representing the possible mental operations as propositional in each case; but what is more important is that he gave no indication of what behaviour the rat could perform that he would take as indicative of self-initiated internal operations. That is to say, lacking any behavioural criterion by which he could judge whether or not ideas or any other form of autonomously mental operations took place, Small was constrained by the canon of parsimony to conclude or assume that they did not. As a result, the evidence of the rat's behaviour could not be implicated in the general direction of the interpretation of its mind or consciousness. This general direction of interpretation was established in advance, as indicated, by the methodological constraints on the making of subjective inferences.

The irrelevance of behavioural observations to the general line of interpretation of the animal mind on the one hand, and the polarity of trivial inferred conscious contents versus the great

functional significance they were supposed to have on the other, together were responsible for the estrangement of data from the theoretical interpretations which were supposed to make sense of them. Thorndike (1901) vigorously criticized Small's experiments partly on the grounds of the equivocality and unwarrantability of any conclusions which could be drawn from them concerning the constitution of the animal mind; but Thorndike's own ideas about how to proceed in interpreting the animal mind were not sufficiently clear that they could have very much effect--even assuming, as is dubious, that they could have had much effect otherwise.

It did not take long, nevertheless, for the unsatisfactoriness of this kind of theoretical interpretation to give rise to a widespread reaction. Small's experiments constituted the first detailed working out of the functionalist programme in comparative psychology. In the eight years between 1899, when Small's initial report was published, and 1907, when Watson stopped making use of subjective inferences in his own experimental work, there was only a slight elaboration or exacerbation of the trends which Small's own discussions displayed. In the five years between 1907 and 1912, when Watson repudiated subjective inferences altogether, there was only a ^{little} ~~slight~~ bit more¹⁸. The behaviourist revolution that Watson inaugurated was a direct and largely explicit reaction against the kind of psychological interpretation exemplified by the quotation from Small. The reaction, furthermore, was in light of the imbalances of functionalist comparative psychology a justifiable one, although the direction which the reaction took in being promulgated beyond the bounds of comparative psychology was, perhaps inevitably, such that these imbalances were never rectified.

III. The Birth of Behaviourism.

Contrast between American and British Comparative Psychology.

In brief, the function of functionalist comparative psychology was twofold. On the one hand, it served to assimilate all considerations of mentality and of psychological organization in animals to the private consciousness of individual animals. That is, everything about the animal which is not itself behaviour, but which is presumed to determine or have some effect upon behaviour--in other words, everything related to the animal's mind--has an autonomous status such that it cannot receive any investigation predicated merely upon a study of its presumed effects upon behaviour but requires, in addition, an interpretive key (use of the analogy) that has in itself no evidential justification and no direct relationship to behaviour. The dualism effectively incorporated within the comparative side of functionalism was therefore not just one of mind and behaviour. It was one of a mind which could not receive any characterization merely on the basis of its effects, and conversely, of behaviour which was not sufficient to provide any information about the factors which potentiated it. Mind and behaviour were both inescapably an sich. What mediated between them in the construction of theories was something that belonged to neither, that is, the analogy.

On the other hand, when the analogy was applied to the interpretation of mind, the requirement that the interpretive inferences be minimal rendered the mind thus revealed of such a sort that its separate and special status hardly seemed to have any point. The private consciousness to which all mentality was assimilated, and which was therefore so important in guiding behaviour, turned out to contain little more than a copy of observable external factors. Mind was

an sich but, apart from the fact that it was totally other than and separate from--that is, had a different ontological status than--the organism's immediate environment, seemed curiously similar to it. Behaviour could not reveal the factors which potentiated it, but these factors, again apart from their different ontological status, were almost identical to the external environmental and experimental factors which were in fact the ones systematically manipulated in order to vary behaviour.

This situation may be contrasted with the one that prevailed in British comparative psychology, which also effectively incorporated a dualism of mind and behaviour. There, as much by accident as anything else, mind was not so thoroughly separated from behaviour, and so could be characterized in part on the basis of that behaviour. That is, it was at least partly an accident that this was so, since initially it was the mind of the introspectionists--the mind comprised of conscious contents--that the British psychologists were trying to discover in their animal subjects, just as it was for the Americans. For the British, as a result, those considerations of mentality and psychological organization which did become assimilated to private consciousness fared no better than in America. They likewise required interpretation by means of the analogy, and were, consequently, incorrigibly artificial; but they were not required to comprise the totality of what was being investigated as mind. Because of this, the inability of the British comparative psychologists to discover the mind of the introspectionists--an inability which was quite as unrecognized for them as it was for most of the American researchers--did not lead them into a dead end. Instead, it led them step by step to a mind that they could discover, the mind that manifests itself sufficiently in the organism's adaptations to complex and ever changing environmental conditions.

What gave the British comparative psychologists this relative freedom of maneuver, so that they did not have to assimilate all mentality to private consciousness, was the negative factor of their not being primarily concerned with the relationship between conscious contents and behaviour. Consciousness, for them, was of interest for its own sake even more than for its relation to ongoing behaviour. The demands of objectivity in reconstructing the mind on the basis of observations of behaviour therefore led them, not to trivialize their subjective inferences (which would have contradicted their aims), but to supplement them with objective ones which could provide an apparently sounder footing--it was certainly sounder in the sense of being more detailed, definite, and unambiguous, even if not in the sense of being more conducive to accuracy--from which the subjective inferences could then proceed. Such a method could not fully explicate the relations between consciousness and behaviour, since the variability in the inferred conscious contents and the variability in observed behaviour did not always correspond; but for the British comparative psychologists, if not for the functionalists, that was an acceptable limitation. What such a method could do was to make the description of consciousness both detailed and apparently definite (which was initially its main purpose), and also, almost incidentally--although less and less so as the science developed--to allow sophisticated and exact determination from an external viewpoint of the capacities and mental operations involved in an animal's behavioural adaptations. The mind-behaviour dualism implicit in British comparative psychology was therefore in part--the part that made the development of science possible--a dualism of capacities or faculties vs. their behavioural manifestations, capacities for behaviour vs. the behaviour

which manifests them¹⁹.

For the functionalist comparative psychologists, by contrast, there could be no compromise with their goal of showing how conscious contents guided the behaviour of animals, for explicating that relationship was central to their conception of the task of psychology. Like the British psychologists, the functionalists could subordinate non-essential aspects of their task to the essential ones, but for them the nonessential aspects were the converse of what they were for the British; that is, they were those concerning the intrinsic constitution of consciousness. This intrinsic constitution could be trivialized so long as the relationship between consciousness and behaviour, which relationship was for the functionalists the essential thing, was left intact. The functionalists were thus constrained to attack the problem of that relationship head on, by using the analogy without any additional tool which could support and hence vindicate it. In consequence, they could not begin to develop any fundamentally different ways to account for behavioural adaptations until they were prepared to replace the introspectionist conception of mind with one that was more appropriate to their purposes--which they could not do for a number of reasons, the main one being that for some time the introspectionist conception was the only widely available one that did not carry an unacceptably heavy gloss of Hegelian idealism or theology or both²⁰--or, of course, until they were willing to drop the reference to mind altogether.

It is one of the small ironies of history that the functionalists were unable to deal with the adaptive capacities of mind, precisely because they were trying so hard to do just that; while the British comparative psychologists were able to make considerable progress in the task, precisely because it was not initially their major concern. The

irony is resolved, if not lessened, by the realization that the two groups of comparative psychologists shared a conception of the mind that is totally inappropriate as a basis for investigating the adaptive capacities of animals; but the British psychologists, because they were not so tied to using it for that purpose, could at least begin to transcend it²¹.

The Behaviourist Reaction.

When a dualism proves unacceptable, when the way it divides nature comes to be regarded as counter-intuitive or counter-productive of further intellectual and scientific progress, the wisest course might then be to re-examine the resulting division, to see if what has been treated as two might better be regarded as one, or if the lines of division should be differently drawn. However, this course is hardly ever followed, or if it is followed by some individuals it is with little effect. The more typical reaction to an unacceptable dualism would seem to be to emphasize one of its poles to the exclusion of the other. Thus, to Descartes' dualism of mind and body, a dualism that had a primary metaphysical and a subordinate epistemological aspect, the metaphysical reaction was La Mettrie's mechanistic materialism and the epistemological reaction was Condillac's sensationalism. Neither was able fully to replace Cartesian dualism because each proved unable to incorporate many central aspects of human experience to which the Cartesian model, in however unsatisfactory a manner, gave due weight.

Something similar happened with the founding of behaviourism. The dualism of mind or consciousness vs. behaviour that was the unwitting product of functionalist comparative psychology was clearly untenable. The mind which resulted was autonomous and unavailable to independent empirical investigation, was somehow possessed of enormous

functional significance while being comprised of trivial contents. The behaviourist reaction was to drop the reference to mind or consciousness and to try to assimilate all psychological considerations to the behaviour which, in functionalism, was the complement of consciousness.

The foundation of behaviourism as a self-consciously new start for psychology was discussed briefly in Chapter 2. Let us now examine it in more detail specifically as a reaction to introspective psychology and to functionalist comparative psychology, as expressed in Watson's (1913a) announcement of the movement's birth.

Given the conceptual difficulties associated with functionalist comparative psychology, one could well sympathize with Watson's complaint:

On this view, after having determined our animal's ability to learn, the simplicity or complexity of its methods of learning, the effect of past habit upon present response, the range of stimuli to which it ordinarily responds, the widened range to which it can respond under experimental conditions,--in more general terms, its various problems and its various ways of solving them,--we should still feel that the task is unfinished and that the results are worthless, until we can interpret them by analogy in the light of consciousness. Although we have solved our problem we feel uneasy and unrestful because of our definition of psychology: we feel forced to say something about the possible mental processes of our animal (*ibid.*, p. 160).

Watson's recommendation with regard to this unsatisfactory situation was, at first sight, an eminently reasonable one. He suggested that since the relation of consciousness to behaviour is essentially stipulative, experimentally indeterminable, and irrelevant to all those problems that can be investigated experimentally, it is fruitless to continue trying to solve all those problems--pseudo-problems we might almost say--which pertain to the relationship.

Such problems as these can no longer satisfy behavior men. It would be better to give up the province altogether and

admit frankly that the study of the behavior of animals has no justification, than to admit that our search is of such a 'will o' the wisp' character. One can assume either the presence or the absence of consciousness anywhere in the phylogenetic scale without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental attack upon them (ibid., p. 161).

Such considerations as these served to support his contention that we should drop or at least minimize all concern with consciousness in comparative psychology, and study only that which is experimentally investigable, that is, behaviour per se.

It seems reasonably clear that some kind of compromise must be effected: either psychology must change its viewpoint so as to take in the facts of behavior, whether or not they have bearings on the problems of 'consciousness'; or else behavior must stand alone as a wholly separate and independent science (ibid., p. 159).

How promising such a programme would be would have to depend on the characteristics of the 'behavior' which was henceforth to be emphasized, and on what, if anything, was eventually to replace the introspective conception of consciousness. The two considerations are closely related, as the introspective conception of consciousness and the functionalist conception of behaviour (which Watson was starting from) were complementary. As it turned out, however, nothing was to replace the introspective conception of consciousness, and the complementary conception of behaviour was to be left on its own. This became clear as Watson continued his polemic. In the first instance, his analysis mainly supported a demand for the autonomy of comparative psychology. In this restricted field, the focus of attention solely on observable behaviour could be justified by the unmanageability and fruitlessness of the more heavily theoretical conceptions which were available to comparative psychology. Watson went further, however, and his analysis became a bid for assimilation of the rest of psychology

when he carried the argument out of comparative psychology as such and into general psychology. After insisting on the independent value of behavioural observations in comparative psychology, Watson proceeded to detail the conceptual and methodological problems which he considered were an inevitable accompaniment of any kind of psychology, not just comparative psychology, which was dependent in any way on introspection. Introspection was an inadequate method and 'consciousness' as such provided the foundation of an inadequate conceptual framework, not only for comparative psychology, but for scientific psychology in general. Consciousness, therefore, had to be scrapped, without replacement, throughout the discipline.

I do not wish unduly to criticize psychology. It has failed signally, I believe, during the fifty-odd years of its existence as an experimental discipline to make its place in the world as an undisputed natural science...The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation...I firmly believe that two hundred years from now, unless the introspective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' whether intensity is an attribute which can be applied to color, whether there is a difference in 'texture' between image and sensation and upon many hundreds of others of like character (*ibid.*, pp. 163-164).

The existing division of labour between consciousness and behaviour, and hence between introspective and behavioural methods in psychology, was not, it seemed, to be reworked or re-analysed; rather it was to be repudiated. Watson, in other words, was standing out as the champion of that behaviour which he had been investigating all his professional life--the behaviour studied in functionalist comparative psychology--over against the consciousness in terms of which that behaviour had until then been explained--the consciousness likewise studied in functionalist comparative psychology, as well as in introspective psychology. The two poles of the functionalist dualism, consciousness and behaviour, were so far separated

that they could not be related to one another except on a priori grounds. Since the former, consciousness, could have no observational status as a theoretical construct and no independently determinable role in the production of behaviour, the natural response to Watson seemed to be simply that of lopping it off, sundering the dualism and making an enormous gain in conceptual economy thereby. This was Watson's response in emphasizing one pole of the consciousness-behaviour duality to the exclusion of the other, and furthermore, by excluding that other, declaring that all genuine phenomena and problems which previously had pertained to it could be better expressed in the language of behaviour:

Psychology as behavior will, after all, have to neglect but few of the really essential problems with which psychology as an introspective science now concerns itself. In all probability even this residue of problems may be phrased in such a way that refined methods in behavior (which certainly must come) will lead to their solution (ibid., p. 177).

Thus, the behaviourist reaction to the incommensurability of consciousness and behaviour, to the unbridgeable gap between theory and data, to the esoteric distinction between the (hardly distinguishable) internal causes of and the external stimuli to behaviour--the reaction to all of this was to deny the meaning, existence, or conceptual necessity (there was some vacillation) of consciousness, theory, and internal causes; to repudiate, in short, that which could not be resolved. It was all declared unnecessary, throughout all of psychology, with the firm expectation that everything of genuine psychological interest, everything that was in any way related to observable behaviour (including, therefore, not consciousness, but talk about consciousness) could be fully accounted for without it.

This clarion call to behaviourism was certainly revolutionary

enough in its abrupt denial of all concern with consciousness, and it was appreciated as revolutionary from the beginning, since an attack upon consciousness as the subject matter of psychology seemed to strike at the heart of the scientific enterprise. And strike it did, although as an attempt at a direct resolution-through-repudiation of a dualism, it involved no re-examination of the terms of that dualism and only little of the relationship between them, and therefore carried over into the new age a major part of the conceptual framework which that dualism embodied.

In contrasting behaviour as it was then conceived, with consciousness as it was then conceived, and affirming that only the former should have scientific status, Watson was, again, attempting to resolve the consciousness-behaviour dualism of functionalism by denying it and expressing everything of psychological interest in terms of behaviour alone. What initially limits such a programme is that 'behaviour' is not a pristine concept. In the consciousness-behaviour dualism, behaviour as well as consciousness derives its properties from the dualistic relation--just as, in Cartesian dualism, body was assigned its properties by contrast with mind as much as (or even more than) mind was assigned its properties by contrast with body. Behaviour, like body, is thus not given as uninterpreted data, but as the embodiment in a fairly simple way of the conceptual framework to which it has been assimilated. If there is something wrong with the conception of consciousness which was associated with functionalism, there is likewise something wrong with the complementary conception of behaviour, and any programme of research based upon that conception of behaviour could therefore be expected to embody, in a suitably disguised form, the same limitations.

What is wrong with the conception of behaviour which grew out of functionalism is that it is an impoverished concept. Everything that gives meaning to behaviour, everything that establishes behaviour as adaptation to the environment, as subserving the economy of the organism, as performing broadly biological functions--all of these features of behaviour are by no means denied in the functionalist conception, but are separated utterly from the observable aspects of the behaviour of animals, and are assimilated loosely and unclearly to a private consciousness that cannot be characterized merely in terms of the adaptive functions themselves. What is left as observable behaviour is the movements of a physical body in space; such behaviour, again, is insufficient to reveal the factors which potentiate or evoke it. The meaningfulness of behaviour, in short, neither resides in behaviour nor can be characterized solely on the basis of the observable features or results of that behaviour. Without the somewhat ineffable consciousness that, so to speak, 'informs' behaviour, the behaviour of organisms differs little from the behaviour of stones.

This is not to say that a psychology based on the repudiation of consciousness as the guide for behaviour would be wholly unable to deal with the factors responsible for behaviour, or would be unable to distinguish organisms from stones. Consciousness as the basis for behaviour was, as we have seen, a somewhat trivial construct, serving as little more than the means whereby external stimulation could be transformed into internal agency. The way in which this internalization actually resulted in adaptive agency--that is, the way in which the private consciousness of the animal was directed toward the process of adaptation--was never made clear; and indeed, it could not be made clear so long as the methodological constraints on subjective inferences

restricted those inferences to the minimal sensationalistic ones²². With the repudiation of consciousness, therefore, what were left as the determinants of behaviour were the same experimental and other environmental conditions which had already effectively served as determinants through the mediation of consciousness. These external conditions could now be varied explicitly so as to show their effects on behaviour--on behaviour as physical movements--and could thus be shown to account for behaviour by themselves without the necessity of invoking any intermediary, non-empirical, explanatory concepts. At the beginning behaviourism was thus, at the very worst, no worse off for explanatory concepts than was functionalist comparative psychology.

But if all this is not to say that a behaviourist psychology would be wholly unable to account for behaviour, it is still to say that the behaviour which it was capable of accounting for remained the behaviour characteristic of functionalist comparative psychology. The focus of control has shifted, from the environment-via-consciousness to the environment per se, but the behaviour that is thus controlled remains isolated from its biological adaptive significance. The meaning or functional significance of behaviour still does not reside in behaviour, nor does it serve (through the conception of adaptive capacities) as one of the determinants of behaviour; it appears rather as the result of behaviour, as an abstraction from the observed series of environmental-behavioural correlations or stimulus-response connections. Environment has thus replaced consciousness as the vehicle of meaning, but has not improved upon it; the functional significance which is the meaning of behaviour still forms no part either of the constitution or of the immediate determinants of behaviour. The most essential characteristics of the organism's functioning in the world--

previously its consciousness, now its behaviour--remain a reflection of environmental conditions. Watson put it quite clearly; the "principal contention" of behaviourism, he said, is that "there are no centrally initiated processes. The environment in the widest sense forces the formation of habits (Watson, 1914; 1967 ed., pp. 18-19)." Thus, it follows that the goal of a behaviourist psychology--the only reasonable goal under the circumstances--is that "given the response the stimuli can be predicted; given the stimuli the response can be predicted (Watson, 1913a, p. 167)."

It can be seen, therefore, that the behaviourism propounded by Watson was an improvement over functionalist comparative psychology in the one important respect that it made the factors which the latter had in fact emphasized in the determination of behaviour--experimental and other environmental factors--into the explicit determinants of behaviour. These determinants were thus essentially unchanged as to their content but were reformulated so as to make them explicitly manipulable and observable. Behaviour was, consequently, of such a sort that it could now reveal the factors involved in its production, which it could not do in functionalist comparative psychology unless abetted by application of the analogy. But behaviourism was not an improvement over functionalist comparative psychology, or any sort of significant change from it, in its conception of the composition of behaviour. Behaviour remained composed of isolated, separable responses, each occasioned separately by specific causal factors and the entire series integrated likewise by external causal factors. Control, whether naturally by the environment or systematically by the experimenter, remained central to the conception of behaviour--not extrinsically, as related merely to the goals of research, but intrinsically, as following

from the composition of behaviour as being essentially reactive, as being evoked in each particular by environmental pressures. Again, it is because of this composition of behaviour as reactive that Watson could formulate the goal of behaviourism--given the stimulus, to predict the response, etc.; for given that behaviour is reactive in this sense (as previously, consciousness had been), achieving that predictive goal would amount to making a complete explanation of behaviour²³. Behaviourism therefore retained the investigative priorities that had unwittingly developed in functionalist comparative psychology, those of showing precisely how the organism's functioning (behaviour, consciousness) was a reflection of the specific parameters of the experimental situation. Behaviourism replaced the old sensationalism with a new environmentalism, and conscious processes with responses as the unit of analysis, and thereby structured the priorities in such a way that they could be implemented more directly and precisely than before--a significant change, unquestionably, even if it did not involve any fundamentally new problems or solutions. It is in this sense, which is not exactly the one he intended, that Watson was quite correct in his claim that "behaviourism is the only consistent and logical functionalism (ibid., p. 166)."

As such, behaviourism established itself, initially at least, as no more conceptually suited to dealing with problems of generalized adaptive capacities in animals than functionalist comparative psychology had been. Just as the latter had been directed by its research priorities toward explicating the specific subjective experiences implicated in the performance of a given behavioural adaptation, so was the former, inheriting these priorities, directed toward explicating the specific stimulus conditions which called forth a given response. Just as the particularized nature of these subjective experiences made it difficult for the

functionalists to arrive at an effective general characterization of the mental processes required for bringing about behavioural adaptations, so did the particularized nature of these stimulus conditions make it difficult for the behaviourists to arrive at an effective general characterization of the relationship between environmental circumstances and the patterns of adaptive responses made to them. Just as functionalist comparative psychology tended toward sensationalism largely as a result of the rigorous experimental control needed for drawing subjective inferences, so did behaviourism tend toward elementarism largely as a result of the rigorous experimental control needed for the demonstration of stimulus-response connections. In both cases, the artificiality of the fundamental units of analysis (specific subjective experiences, specific responses) tended to restrict the theoretical and experimental analyses to isolated laboratory situations.

The restrictive tendency was resisted of course. It was not the intention of Watson or any of the other early behaviourists to restrict the scope of their explanatory models in any such a way, any more than it had been the intention of the functionalist comparative psychologists to do so. On the contrary, both groups were very concerned to avoid the narrow, abstract, laboratory-based academism which they both considered an undesirable characteristic of introspective human psychology. Both therefore made vigorous attempts to apply their principles to the understanding of concrete practical situations, and did so with varying amounts of at least partial success. Despite these efforts, the tendency toward minute analysis of specific events, toward making the units of analysis smaller and more restricted (rather than larger and more inclusive) as a function of increased experimental control and theoretical sophistication, was part of the methodological foundation of both

movements; and it was therefore a tendency, central to both movements, that required constant resistance in order for either to make any continuing progress toward their explanatory goals. The tendency toward minuteness of analysis at the expense of breadth of coverage, it should be emphasized, affected both functionalist comparative psychology and behaviourism equally; it stemmed as has been shown from the emphasis on rigorous experimental control, characteristic of both movements, rather than merely, in behaviourism, from an emphasis on observable behaviour.

Thus, from the viewpoint of behaviourism's relationship to the psychological tradition from which it emerged, as well as from that of its specific proposals for the future conduct of psychology (cf. Chapter 2), the revolution that founded behaviourism was a methodological revolution. Behaviourism consisted largely in an objective reconstruction of the programme of functionalist comparative psychology--or, as we can now say, an objectivist reconstruction, since what was central to it was the replacement of subjective with objective formulations as a matter of principle, based on the judgment that only the latter should be admissible in science. The repudiation of consciousness, the adoption of a stimulus-response model, the tendencies toward environmentalism and minute analysis --all of these characterized the foundation of behaviourism, and all stemmed from the simple elimination of subjective inference and the consequent objectivist reconstruction of the programme and conceptual framework of functionalist comparative psychology.

Alternatives to the Behaviourist Reaction.

We may pause for a moment to inquire whether the reaction against functionalist comparative psychology could have taken any other form than the behaviourist one. On the surface, it would appear obvious that the methodological orientation of the contemporaneous British work

in the field might have provided an alternative--especially as it is somewhat fictional to call this orientation 'British' in any case, one of its chief exponents being Thorndike. Watson, or someone else, might indeed have repudiated 'consciousness' as comprising any part of the subject matter of comparative psychology, and declared (then implemented the declaration) that the subject matter should instead be the capacities or processes of adaptation themselves. Such a reformulation might well have made something similar to the workable parts of the 'British' programme central to comparative psychology, while leaving out the artificial descriptions of conscious experience also implicated in that programme. Furthermore, such a reformulation could have been perfectly in line with the functionalist emphasis on growth and adaptation--far more in line with it than the actual programme of functionalist comparative psychology ever was. Why, then, could no such reformulation as this have been made the basis for change?

It is impossible to do more than speculate as to why such a programme might not have developed, but a few suggestions offer themselves. First, the British orientation was never explicitly formulated (except somewhat elliptically by Hobhouse) as a way in which behavioural evidence could serve, without recourse to the analogy, as the basis for the characterization of mind; such a formulation of that programme depends upon a reconstruction of it as was offered here. The British workers made as thorough and determined use of the analogy as did the Americans, only, it must have seemed, more loosely and imprecisely, and with even fewer doubts about its validity. What was viable in their programme was thus never available as an explicit alternative to the functionalist procedure. Furthermore, even if it had been so available it might well have been unacceptable; formulated explicitly, the British

programme was very similar to a faculty psychology, and faculty psychology was in even worse repute in America (where it had been appropriated by the parsons) than in Britain (where it was merely suspected of being unscientific--cf. footnotes 19 and 20 to this chapter). Watson's concluding hope in his announcement of behaviourism was that a behavioural psychology would soon be "as far divorced from an introspective psychology as the psychology of the present time is from faculty psychology (ibid., p. 176)."

Basically, however, it may be that such a reformulated programme would have been at once too much of a change and too little. It would have been too much of a conceptual reorientation to take effect entirely within the introspectively derived framework of functionalism, and too little to serve as the basis for a strong reaction against it; too much in that it would seemingly have required the abandonment of the whole concept of analytic introspection for insufficient reasons, and too little in that it would have kept the unobservable inner mental causes of behaviour intact. It would have required a subtle and sophisticated reanalysis and reformulation of the functionalist dualism; and as suggested previously, such a reanalysis of a dualism is both difficult and rare.

This speculation is reinforced by the fact that some such reformulation was indeed attempted, and for the whole of functionalism, by Dewey (1896; cf. footnotes 13 and 16 to this chapter). Dewey's intent was to stress the coordinated reaction of the individual to events constituted as such by way of their impingement on the individual's life situation. Stimulus, response, consciousness, were not, Dewey maintained, isolated events or states, but components equally of an integrated adaptation; separation of them by analytical introspection or by any other means was artificial and destructive of the integration. However,

Dewey's analysis, while often cited, was never incorporated within the conceptual basis of functionalism, and Dewey himself did not long remain involved with psychology as it was practiced by the psychologists.

Thus it may be that Watson, and with Watson those who followed his lead, were at once too much trapped within the already constituted functionalist conceptual framework, and too impatient with the experimental products of that framework, to consider making basic modifications of it. The implicit aspects of that framework--elementarism, experimental rigour--had become too fundamental to their conception of behaviour to be seriously modified; and the explicit aspects--conscious processes, internal causes of behaviour--had become too unacceptable to be retained. If this reconstruction is valid, then it would have been, not quite impossible, but certainly very difficult, for the behaviourist reaction, at least within comparative psychology, to take any form basically different from the one it did, involving a repudiation of the explicit problematic elements and a retention of the implicit methodological ones.

The Incorporation of Positivism.

The account as presented so far has, however, an element missing, one which helped to give the newly constituted behaviourism its initial momentum and to integrate some of its diverse trends. It has been left until the end so that the behaviourist reaction could first be presented, as far as possible, solely in terms of problems internal to the practice of psychology. This remaining element is simply the source of the 'principle' by which subjective formulations could be generally replaced with objective ones, both in comparative psychology and throughout psychology. It is here that the incorporation of positivism within behaviourism appears and becomes relevant, for with this incorporation, all of the features of behaviourism as just cited were free to become

subtly modified.

The incorporation of positivism within behaviourism must itself be regarded in two aspects, as an internal development within comparative psychology and as an external influence brought in for the purpose of extending the range of that development throughout psychology. The repudiation of consciousness as a concept or construct within comparative psychology was perhaps reinforced by the already growing positivist and anti-metaphysical cast of the scientific culture as a whole, resulting from the breakdown of the Newtonian synthesis, but it was nonetheless a specific response to a specific conceptual dilemma primarily affecting one area of psychology. The repudiation of unobservables--the unobservables that were causing trouble--in comparative psychology was in this respect a purely internal development. It was unquestionably a positivistic move, but was one of the sort that was described in Chapter 3 as a natural and appropriate response to unresolvable conceptual dilemmas which come to vitiate scientific research in a given field. Whether, had the reaction stopped there, the positivist recasting of comparative psychology would have eventuated in a new set of theoretical principles and presuppositions which would gradually have been allowed tacitly to assume realistic significance, as was described as the typical course in many other sciences, is impossible to say with any certainty; but there is no specific indication in the course of psychological research to that time which would cause one to assume otherwise. However, the reaction did not of course stop with comparative psychology but was extended throughout psychology. As a result, the purely internal pressures which justified it within the more limited field were no longer sufficient as a justification.

What was required to constitute the behaviourist reaction as

establishing a positivist orientation throughout psychology was the extension of the repudiation of unobservables throughout the discipline --the repudiation of consciousness elsewhere than in comparative psychology and the subsequent repudiation of other unobservables throughout psychology. This extension of the reaction was contained in large part in Watson's original polemic, although it by no means ended there. The conceptual basis for the extension was inherent in the emphasis on rigorous experimental control of environmental conditions as the only means for isolating the determinants of behaviour. That is, there was an implicit environmentalism incorporated within the foundations of behaviourism, one that was entirely analogous to the sensationalism from which it derived, and a tendency toward environmentalism is entirely compatible with an insistence that the determinants of behaviour all be observable. However, the extended version of the rejection of unobservables could not, again, be justified, as the limited form of the rejection of consciousness in comparative psychology could be justified²⁴, by appeal simply to the impossibility of getting on with research while restricted to the old introspective formulations. The general form of the rejection required rather an appeal to the physical sciences as providing an external standard of objectivity against which introspective psychology as a whole could be tried, found wanting, and rejected. In Watson's words:

Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your introspection is untrained. The attack is made upon the observer and not upon the experimental setting. In physics and chemistry the attack is made upon the experimental conditions. The apparatus was not sensitive enough, impure chemicals were used, etc. In these sciences a better technique will give reproducible results. Psychology is otherwise. If you can't observe

3-9 states of clearness in attention, your introspection is poor. If, on the other hand, a feeling seems reasonably clear to you, your introspection is again faulty. You are seeing too much. Feelings are never clear (Watson, 1913a, p. 163).

It was on the basis of nothing more than this application of a hypothetical or ideal external standard of objectivity that Watson was able to carry his argument against consciousness out of its limited ambit in comparative psychology and into psychology as a whole, and to maintain thereby that the unresolved and contentious problems of introspective psychology (e.g., the imageless thought controversy; cf. Chapter 2) would never be resolved, were incapable of resolution. Unless one was "wedded to the system as we now have it," Watson maintained, one could never be expected to believe that "there will ever be greater uniformity than there is now in the answers we have to such questions"---quite regardless of the fact that "it is admitted that every growing science is full of unanswered questions (ibid., p. 164)."²⁵

Thus, to the initial home-grown rejection of consciousness as a theoretical construct in comparative psychology was added its rejection throughout psychology on the basis of appeal to the alleged practices of the physical sciences. It was on this that behaviourism's claim to encompass the whole of psychology was founded, and with this claim, the resulting application of an objectivist, positivist orientation throughout the entire discipline--or at least, as much of it as could be won over. The longstanding tendency in psychology for appeal to the physical sciences, that is, was made the basis for the incorporation (as opposed to an internally stimulated emergence) of positivism, by being used as a standard for the rejection of all unobservables, specifically all mentalistic ones. The adoption of such an external standard of objectivity by which all mental entities could be repudiated was, as we have

seen, consistent with the conceptual framework which behaviourism acquired in its reaction to and outgrowth from functionalist comparative psychology, whereby the emphasis on the environmental control of behaviour established a predisposition toward searching for causes that could be seen, rather than merely hypothesized. That is, it was the establishment of this conceptual and methodological framework that motivated or stimulated the application of the external standard throughout psychology. Since, again, some sort of appeal to the physical sciences had long been customary in psychology, the addition of the external standard of objectivity was therefore not, at the time, a very large step. On the contrary, it is of some interest as indicating the autonomous direction of development of psychology that it was an almost unnoticeably small step. It was a step that had profound consequences nevertheless, because it served to base the restriction of attention to observable entities and processes on a rationale sufficiently general that it could have far wider applicability than any argument derived from particular problems in psychology.

The main effect of the systematic appeal to the external standard of objectivity was to further establish the methodological cast of behaviourism, a cast which it acquired in the first instance through the form of its reaction to functionalism. That is, such a rationale for the repudiation of unobservables, by being more general than any derived from specifically psychological concerns, was also more diffuse in its applicability. The considerations internal to psychology which led Watson to the repudiation of consciousness--the problems of doing research on animal behaviour within a functionalist framework--were related, at least, to what had become a substantive issue, to wit environmentalism. Environmentalism, as basic to the foundation of

behaviourism, established a predisposition toward searching for external and observable causes for behaviour, but these causes were necessarily of a particular sort. Use of the external standard of objectivity, instead of unadorned environmentalist presuppositions, as the basis for the rejection of internal 'mental' causes, made it possible instead for the causes sought to be of any sort whatsoever, so long as they were observable or at least empirically specifiable. Thus, while Watson never came to abandon or seriously limit his commitment to environmentalism--on the contrary, he increased it more and more--he established the methodological base on which others could dispose of it at will.

The full-scale establishment or entrenchment of positivism in behaviourism, through the rejection of other unobservables throughout the discipline, took some time. Unobservables were weeded out of psychology one by one, and by the time they were all or almost all excised it was becoming apparent that behaviourism had to seek a more sophisticated basis than it had possessed previously if it was to continue to develop. The general repudiation was, nevertheless, implicit from the beginning, in the emphasis on the environmental or other control of specific responses, as just discussed. Furthermore, Watson took great pains to eliminate mentalistic terms of every sort from his psychological vocabulary as soon as he propounded the behaviourist programme; his paper on "Image and affection in behavior" (Watson, 1913b) was directed toward this end.

'Instincts' and other concepts indicative of behavioural dispositions were tolerated for a somewhat longer time. They did not become seriously marked for elimination until the mid 1920s, largely as a specific response to their excessive use by McDougall and other

'instinct theorists', who seemed to be using them in the service of the nominalist fallacy. The toleration for instincts in behaviourist psychological theories might have been due in part to the difficulties involved in making a strictly behavioural translation of inherited behavioural dispositions. Even so, as early as 1914 Watson proposed one tentative answer to the problem, in his book on comparative psychology, a book in which he still, in other contexts, made occasional use of the instinct concept:

In thus arguing against a fundamental difference between the behavior of man and brute it must not be supposed that we are trying to support the continuity theory of the Darwinians...It has often been said that where there is similarity in structure, as an observed fact we find similarity in function...Logically we must apply the principle consistently. If we find man doing something which the animal does not do, it is due to one of two things: (a) the animal does not possess the structure, or (b) he does not possess it in a highly enough developed form (Watson, 1914; 1967 ed., p. 321).

Differences in behaviour, in other words, should be accounted for in principle by differences in structure and in environmental conditions, rather than by the presupposition of independently existing and functioning behavioural predispositions²⁶. Again, however, use of this analysis for disposing of instincts would require a continuing theoretical commitment to environmentalism. The course that was more generally adopted was to allow the dispositional properties expressed by the term 'instinct' to survive in a modified form as 'drives'; drives, like instincts, are autonomous behavioural dispositions, but they are different from instincts in that they can be manipulated by the experimenter, thereby satisfying at least the requirements of empirical specifiability and requiring little--eventually requiring almost nothing at all--in the way of theoretical commitment²⁷.

Thus, in taking an external standard of objectivity and

subsequently applying it in blanket fashion throughout psychology, behaviourism assured itself of a generally methodological character; the more pronounced this methodological character became, the less it needed to be tied to any particular theoretical principles or orientations. In establishing its methodological cast on the basis of external standards behaviourism also, not unimportantly, helped to assure its own success, since it was the only movement within psychology that was able or inclined both to meet and to promote these external standards of objectivity; and these standards, for their part, became more and more influential throughout the scientific culture with the increasing tendency toward positivism in the physical sciences and the philosophy of science. In other words, if the environmentalist and methodologically rigorous conceptual framework which behaviourism acquired in its reaction to functionalist comparative psychology provided the thematic background for extending the positivist reaction throughout psychology, the burgeoning influence and popularity of positivist formulations generally in science was in large part responsible for the subsequent success which the extended reaction was to have. Positivism in psychology, in short, was at first the almost incidental (although not unnecessary) addition to an autonomous methodological development in one part of the field; it was initially introduced by way of appeal to an external standard that could provide support for the universalization of that development, and was subsequently maintained and extended as the realization of the dominant and most progressive methodological currents of contemporary scientific thought.

Chapter 5

Implications and Effects of the Incorporation of Positivism into Behaviourist Psychology

I. The Institutionalization and Refinement of Psychology's Positivism in the Transition from Classical Behaviourism to Neobehaviourism.

It is less than clear whether American psychology as a whole can legitimately be said to have been engaged in the context of reconstruction prior to the founding of behaviourism in 1913. It is clear that comparative psychology was so engaged, and furthermore that throughout American psychology there was, beginning around 1900, a gradual increase in the use of objective methods of investigation and a concomitant decrease in the reliance on introspective ones for at least some classes of problems, and particularly for some of those related to human psychology (as mentioned in Chapter 2). This development was, however, broad, diffuse, and evolutionary, much as the earlier shift to functionalism had been, rather than abrupt, revolutionary, or stimulated by any general intractability of current research problems. Conceptual and methodological flexibility was the rule. If, nevertheless, we regard this trend as reconstructive, then we can conveniently fix on the year 1904 as the approximate date of its self-conscious emergence. Affixing such a specific date is of course somewhat arbitrary, but it was in 1904 that J. McK. Cattell made what seems to have been the first systematic statement¹ of the independence, or at least partial independence, of objective experimentation from introspective methods and formulations in psychology.

I am not convinced that psychology should be limited to the study of consciousness as such, in so far as this

can be set off from the physical world...It seems to me that most of the research work that has been done by me or in my laboratory is nearly as independent of introspection as work in physics or in zoology...It is certainly difficult to penetrate by analogy into the consciousness of the lower animals, of savages and children, but the study of their behavior has already yielded much and promises much more (Cattell, 1904, pp. 179, 180, 184).

If the context of reconstruction may be said to have been started with Cattell's paper then, gaining momentum, it continued through the diffuse and uncoordinated but progressively less apologetic analyses of the role of behaviour in psychology, as exemplified by the statements of McDougall (1908), Pillsbury (1911), Thorndike (1911), and others, culminating in Watson's (1913a) polemic. Thereafter there was no doubt of the reconstructive character of the trend, for even if the tendency toward objective methods of investigation had not involved a dramatic recasting of the scope and nature of psychology before Watson's polemic appeared, it certainly did thereafter, as a result of Watson's successful exportation of the problems of comparative psychology, and of the local response to those problems, to the discipline as a whole. From that point, therefore, the context of reconstruction embodied a somewhat more sharply defined direction of progress, resulting from its new focus in the specific conceptual problems of comparative psychology, the response to which, extended throughout the discipline, established the general way in which the reconstruction was going to be made. The reconstruction thus continued through Watson's attempts at translation of mentalistic terms into behavioural ones (Watson, 1913b, 1914); through the adoption of methods of investigation deemed appropriate to the new conception of psychology (Watson, 1916); through vigorous attempts at experimental demonstration of the general worth and broad applicability of the new approach (Watson & Rayner, 1920); through extension of the behaviouristic strictures to include instinct and heredity (Kuo, 1924); and finally, once the initial

battle for objectivity had been won to the extent that Watson could say in 1924 "Most of the younger psychologists realize that some such formulation as behaviorism is the only road leading to science (Watson, 1919; 1924 ed., p. vii)," through the beginning of the settling-down period with concentration on detailed psychological research. It is in this period, starting in the late 1920s, that the context of reconstruction could be expected to give way gradually to the context of construction.

As soon as behaviourism began to settle down in this way, however, it began to become apparent that it did not yet possess all the requisites for the progressive development of the science, that in particular it lacked satisfactory means for the expansion of experimentally adduced results into broader explanatory principles. The conceptual and methodological development of behaviourism during this period has been analysed in some considerable detail by Koch (1959, 1962a, 1964), and the present account will, accordingly, make considerable use of quotations from his statements. Koch summarizes the early development of behaviourism as follows:

Classical behaviorism had been an attempt to escape the stagnation of the subjectivist psychologies then prevailing by providing psychology with a decision procedure, which, it was hoped, would make forward movement inevitable. But though the position soon attained hegemony...it degenerated with comparable celerity into polemicism and inflated program-making...By the late twenties, there was much "objective" experimentation but few bodies of clearly stated predictive principles comparable to the crowning achievement of physics: its theories. Instead, experimentation seemed aimless, "theoretical" hypotheses but loosely related to data, and debate idle (Koch, 1964, p. 9).

In short, behaviourism was sorely in need of some kind of unifying principles that could give it enough direction that its achievements in establishing the objectivity of psychology could be safeguarded and made the basis for somewhat more steady and progressive

theoretical development. The priorities of the emerging context of construction were, characteristically, those of establishing and extending theoretical or otherwise explanatory accounts. Such goals were, of course, always considered the fundamental priorities of the scientific endeavour, in behaviourism as elsewhere, but they had been relatively subordinated during the period of reconstruction, while the foundations of science agreed to be necessary for their implementation were being recast. That job presumably accomplished, the priorities of the context of construction gradually acquired a more central position and stimulated increasing concern with what had to be done in order for behaviourism to acquire increasingly comprehensive theories.

Koch continues:

Neobehaviorism may be seen as a second attempt to provide psychology with a decision procedure--this time an effective one that would conserve the orienting attitudes of [early or classical] behaviorism but recast them in such a way as to give them teeth...The search for a "decision procedure" thus became a search for a formulary of the techniques for "constructing" rigorous theory...Early behaviorism had primarily involved attempts to guarantee the objectivity of the descriptive (first-order) concepts used for empirical data. While not giving up this objective (and indeed trying to place its pursuit on a more secure footing), neobehaviorism sought to realize and implement objectivism at the level of theory (ibid., pp. 9-10).

One might ask at this point, why was it necessary that the development and elaboration of theories within behaviourism be dependent upon obtaining from somewhere a 'formulary of the techniques for constructing rigorous theory'? Why, that is, could the task not be approached in the way described in Chapter 3 as typical and appropriate in the context of construction, through the autonomous elaboration of theories the terms and concepts of which had already been subjected to searching critical examination and subsequent approval in the context of reconstruction? In part, the reason is simply that it was not the terms

and concepts of any proposed theories, old or new, that were so examined in behaviourism's reconstruction of psychology, but the terms and concepts of psychology as a whole. Behaviourism was not founded on theoretical or otherwise substantive principles but, again, on methodological ones; not on knowledge claims, but on claims about how to develop and assess knowledge claims. Part of the answer, therefore, is that at the time when theoretical development became of central concern within behaviourism, there was a paucity of conceptually acceptable theories to develop.

The lack of initially acceptable theories is only one part of the answer however, or one side of the coin. The other concerns what behaviourism had at its centre in place of theories or substantive insights. What it had was its methodological orientation, that is, its insistence on an anti-mentalistic kind of objectivity, rendered universal throughout psychology by the incorporation of an external standard of objectivity and of a resulting low-level positivism relating to psychology's permissible data-base (as detailed in Chapter 4). This primarily methodological orientation toward science was in large part responsible for the paucity of theories available at the beginning of neobehaviourism, because the insistence on the objective and hence observational character of all reported statements militated against going very far beyond them in the development of higher level, necessarily somewhat abstract, theories (cf. the third quotation from Watson on p. 67). Furthermore, the faith in the power of an objective methodology had not been at all shaken by the modest results of behaviourism's first fifteen years. The natural response to the dawning recognition that higher level theories were after all necessary in science was therefore the employment of further objective methods in the development of theories, methods which

could supplement and extend the range of those already in general use at the experimental level. Just as objective methods were to be the guarantee of successful experimentation, so were objective methods to be the guarantee of successful theorizing.

The important systematic significance of this continuing emphasis on methodology, from the standpoint of this monograph, is that it indicates how it was that, within behaviourism, a positivist orientation came to remain central to the scientific enterprise in the context of construction. The factor that made it possible for it to do so was simply that positivism was from the beginning taken over as the foundation of the movement, rather than occurring solely as an internal development with local significance specific and appropriate to the problem context in which it arose. Later in this chapter we will examine the general sorts of circumstances in which a positivist orientation can be expected to arise as an internal development within a scientific field, provide the dominant approach to scientific inquiry within that field for a time, and then fade away. One example of such circumstances has already been given in detail, in the last chapter: positivism arose autonomously within comparative psychology, as a consequence of the insoluble methodological and conceptual dilemma in which that field found itself; but instead of having merely local significance was constituted as central to behaviourism--to behaviourism as encompassing all of psychology --by being immediately exported from comparative psychology to the rest of the discipline.

With positivism thus central to the behaviourist approach to psychology, the emphasis on methodology and on the observational status (or later, observational anchorage) of the concepts used in any proposed explanatory accounts did not diminish as a result of concentration on

theory in neobehaviourism. Instead, the emphasis was if anything increased, while being made more subtle and sophisticated; given the central position of the positivist orientation as basic to behaviourism's claim to encompass all of psychology, it is difficult to see how the movement could have continued to develop in any other way. Thus, looked at in one way, the emphasis in neobehaviourism on developing a 'formulary of techniques' for theory construction was the natural development and elaboration, within the context of construction, of the positivism that was already established as central to the movement. Looked at in another way, this emphasis, in the absence of any substantive principles, was a compromise between the accepted necessity for theory (if the predictive and explanatory goals were to be met at all) and the demand for objectivity; use of such a formulary constituted the only permissible way in which theories could be developed and elaborated in neobehaviourism, that is, by being kept entirely consistent with and subordinated to the requirements of the extant positivist methodological orientation. But whether the emphasis on techniques of rigorous theory construction in neobehaviourism is considered the natural course of development of the movement's positivism, or a compromise that had to be effected if theories were to be developed at all--the distinction may be mainly a verbal one--it remains clear in either case that the continued adherence to positivism within the context of construction occurred as a result of the central role which that orientation had acquired, from the beginning, in the constitution of behaviourism.

For these reasons, therefore, the search for objective methods of theory construction was the first major task for neobehaviourism. In fact, the search was not at all a difficult one. It met with apparent success almost at once in the formulations of theoretical

structure being advanced within logical positivism and independent, similar movements. Koch continues:

In pursuit of these ends, psychology did not go directly to physics but turned instead for its directives to middlemen. These were, for the most part, philosophers of science (especially logical positivists) and a number of physical science methodologists who had been codifying a synoptic view of the nature of science and who, by the early thirties, were actively exporting that view from their specialties to the scholarly community at large. The view was based on a "rational reconstruction" of a few selected formulations in theoretical physics and put forward a detailed model of the scientific enterprise which came to be known as the "hypothetico-deductive method" (ibid., p. 10).

It was not entirely coincidental that such rigorous formulations of the nature and structure of scientific theories were being advanced by philosophers of science just at the time when behaviourism needed them. Logical positivism was itself part of the generally positivistic reaction to the scientific ferment stimulated by the collapse of Newtonian theory, just as (to a lesser extent, and at a greater remove) behaviourism was. Furthermore, there was at least some slight direct influence of the early versions of behaviourism on the early versions of logical positivism. Bergmann (1954) cites classical behaviourism, along with relativity theory, Poincaré's conventionalism, and the Principia Mathematica, as among the chief sources of inspiration for the members of the early Vienna Circle in the middle and late 1920s, as indicating jointly the relevance, appropriateness, and probable acceptability of formal empiricist analyses of theoretical structures. Thus, in a small way, behaviourism, through its initially positivist cast, contributed to the development of logical positivism--a contribution that was soon to be more than reciprocated by the logical positivists.

Koch briefly indicates some of the elements of the 'new view' of science which resulted from the labours of the logical positivists and

scientific methodologists.

This "new" view held forth an ideal of rigorous theory and seemed to define a route toward its achievement. In barest outline, it asserts theory to be a hypothetico-deductive system. Laws or hypotheses believed fundamental are asserted as postulates, and the consequences of these (theorems) are deduced by strict logical and mathematical rules. The theorems are then to be tested by experiment. Positive results increase the probability of the hypotheses; negative results call them into question. Scientific theories differ from logical and mathematical systems only in that their basic terms are given empirical reference (made to describe the world) by operational definitions (Bridgman) which state the observational conditions under which the terms may be applied. A science aims toward explicit and, if possible, quantitative hypothetico-deductive organizations of events in its domain (Koch, 1962a, p. 401).

And in summary:

In broad aspect, neobehaviourism may be seen as a marriage between the orienting attitudes of classical behaviourism and one or another interpretation of the "new" model of science. The general orienting attitudes are to be implemented by translation into theory, or theory-like formulations, in accord with the requirements of the model. As a result, the earlier attitudes are reasserted but in altered form. Thus, for instance, re objectivism, the metaphysical overtones of classical behaviourism are, at least by frequent asseveration, sloughed off and attempts are made in a variety of directions to find rationales for a consistently methodological objectivism (Koch, 1964, p. 12; italics in the original).

The relationship of the orienting attitudes of classical behaviourism to the 'new view' of science which neobehaviourism came, in the minds of many, to exemplify, should be elaborated. If the shift to the 'new view' of the role and structure of theory was potentiated by the initially positivist constitution of behaviourism, as indicated above, then it is desirable to specify what became of that initially positivist constitution. In other words, the question arises as to how much continuity there was in behaviourism's positivism throughout the movement's development, and how that continuity was expressed.

In fact, the amount of continuity was considerable. In the

transition from classical behaviourism to neobehaviourism what was retained from classical behaviourism was the feature that was central to the initial positivist orientation of the movement, that is, the in-principle repudiation of unobservables as such, expressed as an insistence on an uncompromisingly public and objective observation base. Koch summarizes the neobehaviourist continuation of this insistence as consisting in a pair of requirements relating to the permissible constitution of independent and dependent variables in psychology, and especially for the permissible constitution of 'stimulus' and 'response'. While not attempting to identify the systematic basis for this insistence, Koch indicates also the extent to which it was both prior to and more fundamental than any particular meaning criteria or meta-theoretical principles associated with the 'new view'.

Though interpretations of technical meaning criteria imported from the philosophy of science were free and various, certain core beliefs concerning the legitimate observation base for psychological statements were common to all of them. It is significant that these commitments were historically prior to the importation of such criteria, and after importation, they remained untouched by the frequent and radical changes in meaning theory which continued in normal course of professional epistemological scholarship.

Such rock-bottom commitments concerning the observation base may be suggested via the following reconstructions:

1. All lawlike statements of psychology containing dependent variables not expressible in, or reducible to, publically verifiable and thus "objectively" observable behavior indices are to be excluded as illegitimate... The prototypical case of an admissible dependent variable is, of course, the notion of response or, more specifically, a "measurable" index of response, in some one of the varied, if often unspecified, meanings of "response."
2. Similarly, it is demanded that legitimate independent variables of psychology designate references which can pass the test of independent, simultaneous observability and are definable in either the observation language of physical science or the concepts of physics. The prototypical case of an admissible independent variable is, of course, the notion of the stimulus, again in some one of many rather unseparated meanings (*ibid.*, pp. 13-14; italics in the original).

Nevertheless, despite the considerable continuity manifest, there was a subtle--but far reaching--change in the character of behaviourism's positivism, resulting from the transition to neobehaviourism and hence from the alliance with logical positivism. As we saw in the last chapter, positivism was initially introduced into behaviourism--or rather, behaviourism was initially constituted as positivistic--through appeal to the standards of objectivity and observability allegedly characteristic of the physical sciences. Such standards were taken to provide justification for the exclusion of anything mental from consideration in psychology, because mentality and consciousness could not be observed. However, there was a slight and at first almost unnoticeable difference between what was required for behaviourism to encompass all of psychology, and what it obtained by its appeal to an external standard of objectivity. What it required was a basis for repudiating mind and consciousness. What it got was a basis for repudiating unobservables as such.

The difference between what behaviourism needed and what it got was not particularly significant throughout the period of classical behaviourism, lasting until the late 1920s--after which time, the needs of behaviourism were no longer the same in any case. As long as the insistence was on the "objectivity of the descriptive (first-order) concepts used for empirical data", so that everything that was going to be talked about in psychology had to be the kind of thing that could be pointed to, the repudiation of consciousness and the general repudiation of unobservables went hand in hand, the former being merely a special case of the latter.

However, the transition to neobehaviourism and the rendering explicit of behaviourism's positivism were marked by two trends which

had the effect of notably liberalizing the anti-mentalistic stand. First, things that could not be pointed to were agreed to be worth talking about sometimes after all, so long as the talk was sufficiently careful. Second, the general positivist orientation including the repudiation of unobservables, which had initially been introduced in support of behaviourism's anti-mentalism and which had therefore been effectively subordinate to it, became dominant within behaviourism, independent of its polemical background, and elaborately articulated by a wide variety of theorists from a diversity of theoretical backgrounds. The repudiation of mind and consciousness thus could no longer be considered given, and sufficiently justified by the summary judgment that such things were unobservable. It would have to be based instead on a demonstration that any given conceptualization of mind or consciousness, or of concepts traditionally taken as indicative of them, could not satisfy the empiricist meaning criteria which were now to be the sole arbiters of a concept's admissibility in science.

Now, such a demonstration would be very difficult to provide, in fact impossibly so; there could be any number of possible conceptualizations of mind and consciousness, and a specific demonstration could not be sure of establishing the inadmissibility of all of them. Furthermore, by the time the positivist orientation of behaviourism had become explicit, so that such formal meaning criteria were becoming to be accepted for use, there was little obvious point in continuing to wage the fight against subjectivism in most of its aspects. Introspective psychology was rapidly disappearing from the American scene and the dominance of some form of behaviourism seemed assured. It was thus no longer clear that the continued demonstration of the inadmissibility of the mental would be worth the effort and frustration which it would

undoubtedly involve. Furthermore ~~still~~, if some proposed conceptualization of mind or consciousness were, against all odds, to satisfy the empiricist meaning criteria, could it, in good faith, be rejected as inadmissible nevertheless? Clearly it could not, not at least if the empiricist criteria were to be taken seriously as providing the explicit standards of objectivity--as they had to be, if they were to be used as the basis for theory construction. The possibility thus existed that some refined version of the criteria which had been imported into psychology specifically in order to exclude the mental, might eventually come to ratify mentalistic concepts. But after all, how much harm would really come from such an eventuality? The goal of behaviourism was to make psychology an objective science; if mind and consciousness could somehow be rendered objective, then surely mind and consciousness in that form had to be fit subjects for science. They would certainly bear little relation to the indefensibly subjective mind and consciousness which were studied in psychology before the advent of behaviourism.

This reassessment of the possible status of mind and consciousness, or in general of what was to be excluded from psychology and how, had definite advantages. It seemed to acquit behaviourism of the charge that it was paradoxical and utterly opposed to common sense in that it tried to argue for the nonexistence of something with which everybody was directly acquainted through personal experience. It put the burden of proof on the 'other side', so that behaviourists no longer had to concern themselves with finding ways to keep mentalistic concepts out of psychology; instead, the 'mentalists' were constrained to find proper ways to bring them in.

The in-principle rehabilitation of consciousness--that is, the declaration that it was no longer absent from psychology in principle

but only in fact--was sounded forcefully by Hull in a frequently quoted passage from his presidential address to the American Psychological Association. The "miniature theoretical system" referred to is a very early version of what was to become the Principles of Behavior.

What, then, shall we say about consciousness? Is its existence denied? By no means. But to recognize the existence of a phenomenon is not the same thing as insisting on its basic, i.e., logical priority. Instead of furnishing a means for the solution of problems, consciousness appears to be itself a problem needing solution. In the miniature theoretical system, no mention of consciousness or experience was made for the simple reason that no theorem has been found as yet whose deduction would be facilitated in any way by including such a postulate (Hull, 1937, p. 30).

And then, concerning the in-principle admissibility of consciousness to the body of science, Hull went on:

There is, however, no reason at all for not using consciousness or experience as a postulate in a scientific theoretical system if it clearly satisfies the deductive criteria already laid down. If such a system should be worked out in a clear and unambiguous manner the incorporation of consciousness into the body of behavior theory should be automatic and immediate. The task of those who would have consciousness a central factor in adaptive behavior and in moral action is accordingly quite clear. They should apply themselves to the long and grinding labor of the logical derivation of a truly scientific system (ibid., pp. 30-31).

This statement of Hull's may be taken to mark the final emancipation of behaviourism from its explicitly anti-mentalistic background. The first and the greatest principle of behaviourism, its rejection of the mental, was henceforth to be subordinated to the explicit procedures designed to ensure objectivity in general. It is, of course, quite true that such procedures seemed sufficient to ensure the exclusion in fact of mentalistic concepts for some time. Referring to the "several centuries" of effort on the part of psychological and philosophical theorists to establish the "priority of consciousness or experience", Hull concluded:

Considering the practically complete failure of all this effort to yield even a small scientific system of adaptive or moral behavior in which consciousness finds a position of logical priority as a postulate, one may, perhaps, be pardoned for entertaining a certain amount of pessimism regarding such an eventuality (*ibid.*, p. 31).

The general characteristics of neobehaviourism's brand of positivism having been established, let us return to further consideration of the elements of the 'new view' of science through which the positivism was implemented. Hull was, of course, one of the outstanding methodologists and chief advocates of hypothetico-deductive theorizing in psychology. But if the hypothetico-deductive model of theorizing, as he expressed it, was in some ways at the heart of the 'new view', it was nonetheless not all or even most of what there was to it. The 'formulary of techniques' for theory construction included an extensive apparatus of logical tools for the refining and testing of concepts and theories; although these could be used in conjunction with, and hence be supportive of, the hypothetico-deductive model, they could also be used in virtual independence of it. As a result, they gained widespread use even amongst those theorists--in fact the vast majority of neobehaviourists--whose commitment to the hypothetico-deductive model in all its grandeur was, because of the cumbersomeness of that model, somewhat less than complete. These logical and methodological tools included operational analysis (or definition), empiricist meaning criteria, related empiricist validation criteria for the assessment of the validity of hypotheses and other statements (whether formally deduced from a theory or not), and others. All of these techniques, or decision procedures, could be and were taken as sufficient (i.e., even without invoking the hypothetico-deductive model) to indicate not only how to evaluate statements and theories in science, but also how to put one's ideas into a form suitable for such evaluation.

Perhaps the most important single methodological tool utilized by the neobehaviourists, and also the one that was closest to being home-grown, was the intervening variable. Intervening variables were formally introduced by Tolman in a paper published in 1936, but in fact he had already made extensive use of them in his 1932 book, Purposive Behavior in Animals and Men. The use of intervening variables was taken to establish a way in which purely objective reference could be made to unobservable inner states and processes, by defining such states and processes as consisting in the regularity of the connection between independent stimulus variables and dependent response variables. The specification of the independent and dependent variables, repeated observation of which establishes the presence of the inner state (or rather, indicates the conditions governing the use of the terms which otherwise would be taken to refer to such an inner state), thus constitutes a kind of operational definition of such states and processes. Tolman, in fact, called his approach 'operational behaviorism' before he introduced the term 'intervening variable'. Intervening variables differ from Bridgman's operational definitions, in that while Bridgman's involve a statement of the operations used to measure the quantity of something, intervening variables involve a statement of the observations used to define its presence. Tolman's usage was sufficiently influential that it came to provide the model for operational definitions generally in psychology (e.g., as described by Stevens, 1939); the systematic difference between his usage and Bridgman's was not pointed out for some time (by Israel & Goldstein, 1944). Intervening variables came to comprise the dominant vehicle for the introduction of theoretical constructs, and were extensively used by Guthrie and Hull and their students, as well as by Tolman and his. Even Skinner, who disapproved

of theoretical constructs on principle, made some use of them in his first (1938) book.

The implementation of the various logical techniques considered necessary and appropriate for theory construction into the ongoing course of neobehaviourist theorizing, in such a way as to preserve the original commitment to data-base objectivism while simultaneously de-emphasizing the dogmatic anti-mentalism originally associated with it, was gradual and somewhat piecemeal, but apparently inexorable. It was characterized equally by abstract analyses of the methodological and logical tools involved in theory construction (e.g., Bergmann & Spence, 1941; Stevens, 1939); by elucidatory analyses of psychological concepts and theories on operationist and other, compatible, principles (e.g. McGeoch, 1935; Stevens, 1935a, 1935b; and at some remove, MacCorquodale & Meehl, 1948); and most significantly, by the increasing day to day use of operational definitions, axiomatized theoretical structures, high level theoretical variables with only indirect observational reference (intervening variables), etc., in the experiments and theories of the time (e.g., most notably Hull, 1943; Hull et al., 1940; Tolman, 1932--but these were only the exemplars of what was significant precisely because it was a general trend). Koch states:

The neobehaviorist period was ushered in by Hull's advocacy of hypothetico-deductive method [1930]...Though Bridgman's work had been cited by H. M. Johnson as early as 1930, it was not until the mid-thirties that a spate of articles on "operational definition" directed the attention of psychologists to empirical definition and produced the widespread impression that objectivism could be finally implemented only by careful "operational" practice. It was not until the late thirties that the preceding contexts of discussion were supplemented by analyses which explicitly took the logical-positivist model of science as regulative. Though initially recommendations of axiomatic method and discussions about operational strategy had tended to occur in somewhat separated contexts, both of these topics found an integrative framework in the formulations of logical positivism. Discussions and applications of positivistic meaning

criteria began to appear in the literature side by side with operationist analyses (Koch, 1964, pp. 10-11).

This, then, was the general character and composition of behaviourism's positivism in the neobehaviourist era, or what Koch calls the 'age of theory', an age, that is, which the emphasis on theoretical development justifies assigning to the context of construction. That positivism was autonomous, formal, and explicit; it was free of the more obviously dogmatic anti-mentalism characteristic of classical behaviourism, and was directed by means of the various logical techniques associated with it wholly toward the progressive and objective development of psychological theory. It seemed, furthermore, to be well on the way toward accomplishing these aims. That is, the assimilation of the positivist principles or techniques for elaborating and evaluating theories, their gradual diffusion throughout the scientific culture within psychology, and their eventual synthesis into a more or less established, if not completely unified, conception of science, were widely regarded as satisfying neobehaviourism's need for a logical and methodological apparatus that would ensure its cumulative and progressive scientific character. Koch speaks of the "hypothetico-deductive prescription" as the regulative or normative aspect of these principles or techniques, that is, as the prescription that the more or less established conception of science which they embody constitutes the way in which scientific investigation can most effectively --and in practice therefore should--be conducted; and he characterizes the "age of theory" specifically as the optimistic period (c. 1935-1950) during which this prescription was, in somewhat different ways by different theorists, most widely and cheerfully accepted. He describes the general effects of the adherence to this prescription on the practice and 'self-image' of neobehaviourism² as follows:

The acceptance of the hypothetico-deductive prescription had important consequences for the prevailing conception of the aims of psychology, the conception of where psychology stood in relation to its aims, and thus the indicated route for further progress. It was, for instance, assumed by many that a backlog of significant empirical knowledge existed adequate to the "construction" of broad-scope, if not comprehensive, theories conforming to the requirements of the hypothetico-deductive model. It was believed that psychology was at a stage such that theoretical differences would inevitably and almost automatically be resolved by the "differential test" of "derivations" from rival "postulate sets." Perhaps of most serious import for the character of actual practice was a cluster of beliefs to the effect that adoption of the forms of the hypothetico-deductive method (or the imagery of its forms) guaranteed that the scientific enterprise would be "self-corrective." Such beliefs led, for instance, to the strange expectation that the initial plausibility of a "postulate" is of little moment in that proper adherence to the forms of the hypothetico-deductive method would almost certainly refine its adequacy or lead to its early demise (Koch, 1959, III, p. 777).

The chief purpose of the remainder of the present chapter is to assess and comment upon the in-principle adequacy of the kinds of techniques incorporated within neobehaviourism for the attainment of the ends for which they were introduced, that is, for the establishment of the progressive and self-corrective character of the scientific enterprise as described (with a touch of sarcasm³) in this last quotation from Koch. Or rather, the purpose is--as promised in the conclusion to Chapter 2--to demonstrate the inadequacy of such techniques for attaining these ends, by means of a review of the systematic limitations that have become apparent throughout the last twenty to thirty years on the applicability of these techniques, and, that initial purpose accomplished, to draw some of the implications of their inadequacy.

It is the 'in-principle' rather than the 'in-fact' adequacy of these techniques that is of concern, for two reasons. The first is that there is no question about their in-fact adequacy. The decline of behaviourism, its failure to accomplish its systematic scientific goals,

and the continuing fragmentation of neobehaviourist theory throughout the highly optimistic age of theory and beyond, all provide a clear enough demonstration that the implementation and consistent use of these techniques did not suffice to establish the progressive, cumulative, and self-corrective character of the scientific enterprise.

Koch has marshalled impressive testimony as to this specific point, concerning the in-fact inadequacy of these techniques, from the later reflective writings of many neobehaviourist theorists themselves. Such testimony serves as evidence, not of the inadequacy as such of the techniques, but of the extent to which their inadequacy came eventually to be appreciated by those who had been responsible for their dissemination throughout psychology. The last quotation above is taken from Koch's epilogue and report of trends as revealed in the first 'study', comprising the first three volumes, of his Psychology: A Study of a Science (Koch, 1959, 1962b). This first study brought together 36 eminent theorists associated with a wide of variety of systematic formulations in sensory, perceptual, physiological, and social psychology, and in learning, general behaviour, personality, and psychoanalytic theory; learning theory, for instance, was represented by Estes, Guthrie, Logan (for Hull and Spence), Miller, Skinner, Tolman, and others. These theorists made detailed statements of the development and current status of their systematizations, and analysed the role and contribution of the hypothetico-deductive prescription and its various independent exemplifications (operational definitions, intervening variables, empiricist meaning criteria, etc.) in the development and completion of these systematizations. Immediately after detailing the hoped-for consequences of adherence to the prescription, as quoted above, Koch observed, summarizing the various theorists' positions:

...we may report that the trends of the study are in definite contrast to the earlier Age of Theory position... Few authors in this study would "scrap" the hypothetico-deductive model as the stipulation of a methodological ideal, ultimate approximation of which would be highly attractive. Most, however, would challenge the feasibility of the hypothetico-deductive prescription...as an immediate program for all domains of systematic effort, or indeed for any systematic enterprise contemplating reasonably broad empirical reference (Koch, 1959, III, p. 778; italics in the original).

The second reason for concentrating on in-principle rather than on in-fact adequacy is a more fundamental one, and has been implicated in the discussion at several points throughout this monograph. The claim being advanced here is stronger than the one just reviewed, to the effect that the implementation of a positivist orientation, through adherence to the hypothetico-deductive prescription and the various decision procedures associated with it, did not suffice as a basis for behaviourism to reach its scientific goals. Rather, the present claim is that the implementation of positivism as the basis for elaborating theories was sufficient to prevent behaviourism from thus being successful, largely by ensuring the continuing fragmentation of neobehaviourist theorizing as detailed in Chapter 2. The claim is thus that a positivist orientation is generally inappropriate as a basis for theoretical systematization in a science. It is therefore necessary to demonstrate that the implementation of positivism could not, even in some ideal case, have eventuated in a body of systematized scientific knowledge significantly more comprehensive and integrated than that which in fact resulted from neobehaviourism.

These considerations make it necessary that the analysis focus mainly on the decision procedures and related techniques as originally formulated and extended by 'philosophers of science and physical science methodologists', rather than simply as incorporated into neobehaviourism.

The way in which they were incorporated into neobehaviourism is in most respects quite consistent with the spirit of their development, even if their application did not keep pace with all the modifications eventually made to them. Nevertheless, one could maintain that part of the reason for their failure in neobehaviourism was that they were not incorporated and applied with the necessary skill, sophistication, and subtlety. Koch, in fact, takes such a position; while he is highly dubious about whether the positivist-neobehaviourist programme could have been successful under any circumstances, he feels strongly that it was vitiated in particular by the lack of competence in the way it was actually implemented. Concerning the variety of sources from which neobehaviourism derived the 'new view', Koch states:

It should be observed that psychology's selections from this cluster of formulations were spotty, adventitiously determined, and not supported by especially expert scholarship in the relevant sources...What in fact seems to have been the case is that psychology was enthralled by the apparent authority of these ideas, not their content (Koch, 1964, pp. 10-11).

Koch feels that if the neobehaviourists "had done their outside reading in slightly different order", so that they had discovered Carnap or Schlick before Bridgman, "the clang of the psychological literature re definition would today be different (*ibid.*, p. 24)."

I may briefly state my own position, in contrast to Koch's, on this question of the skill or sophistication with which the neobehaviourists implemented their positivist programme. It is that while the particular components of the programme were indeed introduced in a way that was 'spotty', and while the programme itself never attained full formal coherence, this unevenness was a reflection of the unevenness and lack of full coherence of the positivist principles that were being incorporated, rather than of the level of skill and sophistication with

which the neobehaviourists incorporated them. While the neobehaviourists did not keep up with all the developments in logical positivist thought, and did not wait upon its ultimate stabilization before implementing their programme, they can hardly be faulted for this: logical positivist (and related) formulations kept shifting in a kaleidoscopic manner from the time of their inception, and did not begin to stabilize until, after growing further and further away from considerations of actual scientific practice, they started to disintegrate in the middle 1950s. If the neobehaviourists were going to attempt to implement a formally positivist conception of theoretical science at all, there was no reason, either at the time or in hindsight, for them to delay after first becoming apprised of the possibilities of doing so in the early 1930s. Inevitably, in rushing ahead at that time, they followed a programme that was not so sophisticated in many of its particulars, nor so complete in its formalization, as a later one might have been. But there is a respectable body of opinion, which will be sampled later in this chapter, to the effect that full formalization is by no means necessary as a preliminary to the effective use of the positivist techniques. Furthermore, if, as I maintain, the positivist feature of the neobehaviourist programme were a sufficient cause for the systematic failure of that programme, then any divergence of that programme from a consistent and sophisticated positivism cannot be accounted a necessary component of its failure. On the contrary, such divergence would at worst have no effect on the programme's systematic failure, and at best might mitigate it.

My position on this question cannot, it is true, be justified in all respects. Given that the implementation of a formal or systematic positivist orientation within neobehaviourism was in large part 'spotty' and 'adventitiously determined', it is impossible to say precisely what

the results would have been had the orientation been implemented in a more consistent manner; at least, it is impossible to say whether the failure might then have come about in a somewhat different way. What can be said, however, and what I propose to show, is that given a sincere and whole-hearted commitment to explicit decision procedures such as widely obtained in neobehaviourism, the failure was inevitable; that is, that there is no way in which such consistent implementation of a positivist orientation could have resulted in a successful and cumulative systematization of behaviourist psychology.

Again, therefore, the analysis will have to focus on positivist techniques for the assessment and evaluation of theoretical statements in their most general form, in order to show their basic limitations as vehicles for scientific inquiry. To the fairly small extent that the results of the analysis require further interpretation specifically as relating to neobehaviourism or behaviourism generally, the application will be made following the general analysis. We are therefore taking up the discussion of positivism as a basic for scientific inquiry from the point where we left it at the end of Chapter 3. The discussion was interrupted at that point specifically so that it might be given more substance, by being related to the particular circumstances of behaviourism. We have followed the circuitous and unusual concatenation of influences which eventuated in behaviourism's being founded on a positivist orientation toward the practice and problems of psychology; have characterized the initial form of that positivism; have briefly traced the development of behaviourism to the stage where its positivism became explicit and more or less emancipated from its dogmatically anti-mentalistic background; and have made mention of the significance of neobehaviourism as a unique case study in scientific methodology, a status bestowed upon it by its

unparalleled commitment to a formal positivist programme in the development of theories (cf. footnote 3 to this chapter).

As in Chapter 3, the alternative or antithetical approach with which positivism will be contrasted and compared is that of scientific realism. The aim will be to establish the greater appropriateness of a realist orientation to science in the context of construction--the context in which neobehaviourism operated--and, conversely, the greater appropriateness of a positivist orientation to science in the context of reconstruction. Some initial evidence for or arguments in favour of this position were presented in Chapter 3, but the present account will attempt to establish the conclusion somewhat more firmly.

At first sight, however, this contrast of positivism with realism might seem paradoxical when related specifically to behaviourism. Behaviourism never involved an explicit repudiation of realism, either as a common sense conviction about the potential and ultimate truth of scientific theories, or as a philosophical doctrine. On the contrary, the most influential philosophical defender of the movement in its early stages was E. B. Holt, a member of the important philosophical school known as the 'New Realism'. Nevertheless, although behaviourism was not founded specifically on an antithesis to realism--and although this fact will be seen to be an important one later on--that antithesis is the appropriate one to emphasize in discussing the movement as an implementation of positivism. 'Realism' must be understood as it was characterized in Chapter 3. There, it was shown that a realist orientation to science involves not only an intuitive conviction about the scope of scientific theories but also, following from this and even more important, the ascription to an accepted theory of greater

validity (i.e., potentially ultimate validity) than can ever be warranted on strictly logical and empirical grounds. Neither Holt nor any behaviourist psychologists ever took a realist position in this sense; Holt's realism was something similar to what is known as epistemological naive or direct realism. Scientific realism as characterized in Chapter 3 involves what on positivist grounds would be equivalent to assigning metaphysical status to empirical scientific theories. The repudiation of such a position is implicit in any approach to science which involves adherence to general methodological criteria of objectivity, such as behaviourism was from the beginning; it is explicit both in modern positivist philosophical thought and in neobehaviourism⁴.

A perhaps not unrelated point is that any conclusion that may be reached as to the invalidity of behaviourism as resulting from its positivist orientation will have no direct implications for the value or appropriateness in psychology of what is often taken to be behaviourism's antithesis, that is, some form of mentalism or subjectivism. Although it was in relation to the dogmatic anti-mentalism of early behaviourism that the movement initially became constituted as positivistic, that dogmatic anti-mentalism was itself soon outgrown, having fulfilled its historical purpose, as behaviourism's positivism became more explicit and sophisticated. The residual anti-mentalism of neobehaviourism was given only tentative, matter-of-fact justification by the explicit positivist principles which the movement came to incorporate; and as the quotations from Hull indicated, it was held possible in principle for even this residual anti-mentalism to be abandoned under the appropriate circumstances. On the other hand, if a demonstration of the systematic untenability of positivism in the

theoretical development of science could provide no specific justification for the acceptance of concepts indicative of mind or consciousness, it certainly could provide none for their rejection; and furthermore, it would demonstrate the invalidity of the grounds on which they continued to be rejected 'in-fact' in neobehaviourism. Such a demonstration would be sufficient to indicate, that is, that the putative inability of the majority of concepts indicative or descriptive of mind and consciousness to satisfy formal empiricist meaning criteria cannot comprise sufficient grounds for excluding them from a central place in psychological inquiry.

This example of the kinds of conclusions which could be reached concerning mentalistic concepts typifies the kinds of conclusions which can be reached in general. That is, it will be possible for the negative conclusions, concerning how theoretical systematization in the context of construction can not best be accomplished, to be fairly explicit and definite. By contrast, it will not be possible for the positive conclusions, concerning how theoretical systematization can best be accomplished in the context of construction, to be explicit and definite to any comparable degree. But this limitation will itself emerge as a not insignificant conclusion concerning the role which any methodological principles or analyses, devoid of specific substantive import, can hope to play in the conduct and regulation of scientific inquiry⁵.

II. Realism and Positivism in the Conduct of Scientific Inquiry.

One way of characterizing what is most fundamental to a positivist orientation toward science, to recapitulate what was said in Chapter 3, is to designate it a repudiation of metaphysics or, more specifically, a repudiation of any inferred entities or processes which are unobservable in principle. As we have seen, behaviourism was in

large part initially based on such a repudiation. But while this simple repudiation might be adequate as a basis for ensuring the unequivocal status of statements restricted to the description of observed data, it does not readily apply to the concepts implicated in theories--which are of primary concern, at least in the context of construction--because theoretical concepts as such (e.g., mass, force, reinforcement) rarely designate observables in any simple or direct way. This lack of immediate observability of theoretical concepts was part of the problem faced by behaviourism at the beginning of the context of construction, and it was partly in order to gain ways whereby theoretical concepts could be guaranteed untainted by metaphysics, and hence be suitable for use, that the movement sought advice from the logical positivists.

With regard to theoretical concepts, therefore: to implement a positivist orientation within the context of construction, it would be necessary first of all to develop adequate means to ensure that all the concepts and variables implicated in any proposed theory were free of ontological or metaphysical presuppositions; to ensure, that is, that such concepts are not permitted to refer to any entities or forces which are unobservable in principle. This requirement can be expressed in different ways. In the language of ancient or classical positivism, if a theory is designed solely in order to 'save the appearances', to provide an economical description of phenomena with only 'mathematical truth', it is necessary that independent existence not be ascribed to the mathematical explanatory fictions brought in to account for the appearances. That is, such fictions (e.g., epicycles in Ptolemaic astronomy) cannot be considered to refer to anything except, in a complex way, the phenomena (observed planetary and stellar motions)

which they are used to classify. It can be determined that the explanatory fictions are indeed beginning to function as real or independently existing entities or forces in a theory if, in the elaboration of the theory, they take on or are found to have properties other than merely those which have already been ascribed to them for the explicit purpose of saving or, in a non-causal sense, 'accounting for' the appearances. Thus, it is necessary to have a built-in limitation on the range of operation or extension of the explanatory fictions used in the theory⁶. In the more exact language of modern positivist thought (dating approximately from Mach), this requirement can be reformulated to state that the variables and concepts implicated in a proposed theory must, if they do not have immediate observational reference, have ascribed to them only those properties and functions which can be empirically specified by reference to the class of observable events to which the theory is intended to apply. To put it more simply, the constituents of a theory may be permitted to function in the theory only to the extent that they can be shown to have unambiguous empirical content. One of the main contributions of Mach (1883) to the reconstruction of physics, it will be recalled, was to attempt a reformulation of the concepts of space and time in Newtonian theory in such a way as to eliminate their reference to unobservables.

The problem sometimes arises, however, as to how one can be sure that all the concepts implicated in a theory have empirical content and none other. The Newtonian concepts of absolute space and time, after all, clearly had empirical content. The eventual problems in their application arose because they had non-empirical content as well, and the empirical and the non-empirical components of their theoretical content were not separated. Or rather, the two kinds of content were

not separable at all within the theory, but resulted from the development of the concepts in such a way that their reference extended to empirical and non-empirical, observable and unobservable, entities and processes equally. The conceptual excesses of Newtonian theory which potentiated this indiscriminate reference were, it is true, eventually identified and dealt with; but they came to light only after reliance on them had come to hinder severely the progress of physics, and dealing with them required the overturning of the results of much of the development of physical science during the preceding two centuries. How could the occurrence of such conceptual excesses be prevented from the beginning, before they had such drastic effects? This was the problem facing modern positivist thought.

The answer to the problem seemed clearly to lie in the development of formal meaning criteria, criteria which could be used to evaluate the empirical content or empirical meaningfulness of any proposed conceptualization before it was incorporated into an ongoing theory. It was important that the criteria be formal ones, since informal or intuitive criteria of meaningfulness were obviously unreliable. It could be presumed that Newton had not intended to handicap the ongoing course of science in developing his conceptualizations, and that neither had Helmholtz or du Bois-Reymond when they asserted (see footnote 7 to Chapter 3) that Newtonian concepts were essential to and constitutive of scientific explanation. Since concepts receive their meaning through use (the Newtonian concepts of space and time were empirically warrantable in some of their applications, unwarrantable in others), the meaning criteria which were first necessary were ones which applied to the evaluation of concepts in their simplest applications, that is, to the evaluation of statements. The development of such formal empiricist

meaning criteria for the evaluation of statements, and later of theories, comprised the single most important set of tasks undertaken within the loose alliance of interests that constituted logical positivism.

Some contrast can be seen from the beginning, therefore, between the factors that led modern positivists to develop formal meaning criteria, and those which led behaviourists to incorporate such criteria into their scientific practice. The positivist philosophers and logicians developed such formal criteria in order to ensure that no metaphysical references could be insinuated into scientific theories. The behaviourists, for their part, had effected their own elimination of metaphysics from psychology already, and thus were not so centrally concerned with its excision. Their problem rather was one of how to develop high level theories, given their already established low level positivist commitment to an objective observation base. Thus, they adopted the formal positivist measures, not so much to keep their theories free of metaphysics, as to enable them to develop their theories at all.

Realism and Positivism in the Context of Construction: 1.

Problems in the Development of Meaning Criteria.

The development of the formal meaning criteria judged necessary was originally thought by many logicians to be a relatively simple task. It proved far more difficult than had been expected, however, to develop or construct meaning criteria that would set the boundaries between sense and nonsense, between empirical and non-empirical (or metaphysical) statements, in the right place; to develop them, that is, so that they would allow all statements which were 'obviously' meaningful and disallow all statements which were

'obviously' nonsensical. Such difficulties extended throughout the entire range of the criteria which were first proposed.

To begin with, as described in Chapter 3, it seemed that the verifiability criterion--the first meaning criterion proposed in any detail--condemned itself as empirically meaningless, and hence could have only stipulative significance. In doing so, the verifiability criterion made the same reflexive judgment that Hume's criterion for meaningfulness had made almost two centuries earlier⁷. More important than this however, the verifiability criterion also condemned as meaningless all general or universal scientific laws, since there is no way that universal statements (e.g., "all swans are white", "for every action there is an equal and opposite reaction") can be verified, short of exhaustive enumeration of all possible instances, a procedure which is usually impossible in principle. This particular limitation on the application of the verifiability criterion is a consequence of the practical fallibility and logical invalidity of induction. It might in principle be avoided, therefore, by making use of a criterion of falsifiability, rather than one of verifiability, as the determinant of meaning. Universal statements cannot be verified, but in principle they can be falsified and refuted by a single negative instance; their empirical meaningfulness or their status as scientific statements⁸ can thus, on falsificationist principles, be established by specifying the conditions or events which would constitute their falsification or refutation.

Falsificationist logic has problems of its own however. The exact falsificationist counterpart to the unverifiability of universal statements is the unfalsifiability of existential statements. Existential statements (e.g., "some swans are black") are not falsifiable,

except (as in the corresponding case) through exhaustive enumeration of all possible particulars. To deal with this difficulty, Popper, the chief modern exponent of falsificationism, grasps the bull by the horns and argues persuasively that existential statements, as such, are indeed non-scientific and metaphysical (Popper, 1959). His argument concerning existential statements per se has considerable merit (he assimilates them to statements such as "Somewhere there is a philosopher's stone, a holy grail, a golden isle"); but it does not readily apply to statements containing both universal and existential quantifiers (e.g., "For every pure solid at a given external pressure there is a temperature above which it becomes either gaseous or liquid."--the example is adapted from Maxwell, 1966). Such statements are typically allowable in science, but because of their existential component they are clearly not falsifiable--while conversely, because of their universal component they are clearly not verifiable. It is impossible to examine every pure solid, while if any solid selected for testing resists a change of state, it is always possible that it needs merely to be heated a little more.

There are, in addition, further problems for empiricist meaning or demarcation criteria that bedevil verificationist and falsificationist approaches equally. A trivial example will suffice to show the kinds of difficulties which can arise.

In order to sanction the use of at least some universal statements, logical positivist analyses from about the time of Ayer's Language, Truth and Logic (1936) began typically to emphasize 'confirmability' rather than 'verifiability' as the appropriate criterion of empirical meaningfulness. A statement is confirmable if an observation statement can be deduced from it or from a set of statements of which it forms an essential part. More precisely, a statement M is confirmable

if and only if it can be directly (observationally) verified or if in conjunction with some other statement P it implies an observationally verifiable statement Q not implied by P alone. This confirmability criterion makes allowance for universal statements and for other statements which do not in themselves have immediate observational referents or consequences; but it is intended to require that any such statements have clearly specifiable observational content nonetheless. As it stands however, this criterion renders any statement confirmable, in the following way. Let M be any statement at all, Q any (true) observation statement, and P the statement "M implies Q". Then, from the conjunction of M and P, but not from P alone, we can deduce Q. Q is found to be true by observation, hence M is confirmed. The same trivial example can be used to show that any statement M forms part of a falsifiable set of statements. Let M and P be the same as in the preceding example, and Q any falsifiable statement (it need not be true, nor be observationally falsifiable). Again, the conjunction of M and P implies Q. Thus, specification of the events which would constitute falsification of Q serves also to specify the events which would constitute falsification of at least one of M and P, and hence of their conjunction. It has been shown by Church (1949) that, with somewhat more complicated logical manipulation, any statement M can still be confirmed even if stringent limitations are placed on P so that it must be either analytic, observationally verifiable, or independently confirmable.

Of course these examples are, as mentioned, trivial ones. However, their triviality cannot be taken as an indication that they are irrelevant or unimportant. If it is desired to construct formal criteria of meaningfulness because informal or intuitive ones are

unreliable, then it is pointless to have to resort to informal criteria for deciding when the formal ones are to be applied. Hence, trivial 'mistakes' made through the application of a proposed criterion are relevant precisely because they are trivial. Trivial mistakes are, at least, easily recognized. They demonstrate that the proposed criterion is capable of leading to mistakes. There is, however, no formal criterion of triviality, and no guarantee that naturally occurring mistakes which arise through application of the proposed criterion will be either trivial or in any other way easily detectable. Hence, trivial mistakes serve effectively to invalidate a proposed criterion.

The attempts to develop empiricist meaning criteria continued to encounter difficulties such as these, as well as considerably more recondite ones, to the extent that some logical empiricists (who by and large no longer call themselves logical positivists) came to abandon the attempt to construct formal meaning criteria of any sort (e.g., Hempel, 1950, 1951). Others, recognizing the apparently insurmountable difficulties in unequivocally analysing the meaning of statements, continued to press for the development of formal criteria but came to concentrate on analysis of the meaning or empirical content of theories rather than of statements (e.g., Carnap, 1956, Feigl, 1956). Statements and concepts alike are now widely agreed to receive their meaning as part of the theories in which they are embedded, from what Feigl (1956) calls their "locus in the nomological net". This kind of account no doubt does greater justice to the complexities of the use of concepts in science than did the older logical positivist analyses of statements. However, these more recent attempts at revamping or replacing the old meaning criteria have encountered difficulties of their own. Feigl's (1956), for instance, has not progressed beyond a sketch of the

characteristics which a completed account will eventually have to have. Carnap's (1956), while more complete, allows meaningless contrivances similar to those discussed just above in the case of statements sanctioned by the confirmability criterion. Maxwell (1966) provides as examples of statements admissible to theories on Carnap's criterion, "The cosmic mind dislikes cheese" and (an old exemplar of meaningless statements) "Last night everything doubled in length". He shows also how the meaningless term 'masiquity' can be introduced into any theory through application of Carnap's criterion.

In general, the chief difficulty with any formal analysis of meaning that must be applied to an entire theory is its extreme complexity. A theory which is to receive any such analysis must have, or be reformulated to have, the specific and precise logical structure demanded by the analytic criterion to be applied. The same holds true for the analysis of statements of course; for the simple criteria to be applied, the statements must be of a logical form that could permit verification or falsification, depending on the criterion. Such logical requirements for statements are relatively simple, however, and many statements in science possess them in any case, before any analysis is deemed necessary; the statements "Some cosmic minds dislike cheese" and "All cosmic minds dislike cheese", for instance, have the requisite logical form for application of the criteria of verifiability and falsifiability, respectively. By contrast, the logical form which a theory must have for a criterion such as Carnap's (1956) to be applied to concepts and statements in it is neither so simple, nor so easily obtained, nor so characteristic of already extant theories. As a consequence, it is rather difficult to apply such criteria, and in fact neither Carnap's criterion nor any comparable one has yet been found

capable of general application to extant theories. The upshot of such complexity, therefore, is that there is no longer any cut and dried technique which can be applied to determine whether a given statement or concept, or already existing theory in which these are embedded, can be judged meaningful or meaningless, empirical (in whole or part) or metaphysical (in whole or part). No such complex analysis has come any closer than the simple ones to providing a useable criterion, and indeed it is an unresolved question as to whether such a technique could exist even in principle. That is, it is not certain that it is possible even in principle to give a formal and complete account of the meaning or empirical status of theories or of the terms of them--apart from the apparently insurmountable difficulties of doing so in practice--by means of such global analyses (see, for instance, Maxwell, 1966, for an argument against the possibility of doing so). Unless a proposed meaning analysis is both formal and complete in just this sense, no criterion can follow from it for identifying and expunging the non-empirical components of or additions to a theory that, as a whole, has some empirical content (i.e., entails some confirmable or falsifiable predictions). Making such identification possible for such therapeutic use was, of course, the purpose for which meaning criteria were developed in the first place.

In summary, the development of meaning or demarcation criteria which could be used as tests for statements or theories to prevent the occurrence of conceptual excesses such as those which eventually vitiated Newtonian theory has been effectively frustrated. The early attempts to construct formal criteria for the assessment of statements proved incoherent, and no refinement of them was able to eliminate their incoherence. The few later attempts to construct formal criteria

for the assessment of concepts or statements as embedded in theories encounter the same difficulty, and in addition, the complexity of such criteria and the special requirements imposed by them would render them of dubious generality even if their specific inadequacies were to be overcome. None of the proposed criteria has proved of very much practical value in the task of assessing the meaningfulness of statements or theories.

Realism and Positivism in the Context of Construction: 2.

Problems in the Application of Testing Criteria.

The preceding section showed that it is extremely difficult, if not impossible, to develop theoretical concepts which can be guaranteed to be 'clean', i.e., to have empirical content and nothing else, simply because the criteria for assessing the empirical status of any such concepts have, at least until the present, proven faulty and intractable to improvement. Such problems in the development of meaning criteria do not affect a realist approach to science in the context of construction, for in a realist approach metaphysical baggage (or what positivists might consider such) is not prohibited, and in the context of construction considerations of purity can legitimately be subordinated to considerations of theoretical fruitfulness. Such problems do, however, clearly make it difficult for a rigorously positivist approach even to begin functioning in the context of construction.

Scientific concepts and theories must not only be formulated and judged admissible however, whether they are clean or not; they must also be used. Theories must make contact with nature in the context of construction; they must be tested, developed, accepted or rejected as providing genuine information about the world, however that world is

conceived. Explicit criteria for testing or assessing statements and theories in terms of their validity are much the same as, or can be derived from, those used to assess them in terms of their meaningfulness. The verifiability criterion, for instance, makes the meaning of a statement dependent on a statement of the operations involved in its verification. Testing a statement thereby acknowledged as meaningful simply involved carrying out the specified operations. There are no logical problems involved in doing so that do not equally affect the statement of the operations. Some of the same problems do arise, however, in suitably altered form, so that in the testing and development of theories, no less than in their formulation, formal criteria--chiefly criteria of refutation or falsification in this case, since no-one today would claim that theories can be conclusively verified--are inadequate to the task.

Most of the considerations which warrant this conclusion are well known, and can be discussed relatively briefly here. First, almost every theory encounters anomalies, experimental results and other observed phenomena which are incompatible with the theory. If a scientific theory is regarded (or recast) as a formal hypothetico-deductive system, there will almost certainly be predictions entailed by it which, on any strict application of falsificationist logic (which is not repudiated by any logicians, even if it is supplemented by some), will refute the theory or some part of it. Every scientific theory, as Weimer and Palermo (1973) strikingly describe it, "lives in an ocean of anomalies"; each one is "born refuted". Hull's (1943) behaviour theory, for instance, was 'born refuted' by the results of latent learning experiments (Tolman & Honzik, 1930), and was further refuted in infancy by Crespi's (1944) demonstration of elation and

depression effects on response strength. Newtonian mechanics was born refuted by the failure of the moon to follow the orbit (apparently) ascribed to it by Newton's equations, Copernican astronomy by the absence of any detectable stellar parallax, etc. Instances of such anomalies affect most theories, perhaps all. Their ubiquity may be referred to the incompleteness of our knowledge, to the fact that our best theoretical efforts to date are at best approximations to the truth, or simply to the law of sheer cussedness; such explanations may be consoling, but are not otherwise helpful. For the most part, anomalies must simply be lived with in science.

Reaction to such anomalies in the course of science can take many forms, depending on the specific circumstances of their occurrence and on the inclinations of the affected scientist. Given a theory, and a finding which is anomalous with respect to the theory, should the finding be considered peripheral or central? That is, can the anomaly be safely passed by as indicative only of a unique or uncharacteristic class of events of only tangential relevance to the theory, or does it call for intensive investigation as the key, for good or ill, to the theory's claims to validity? If the latter case obtains, should the theory with regards to which the finding is anomalous be considered wrong or merely incomplete? That is, does the theory need to be replaced or merely extended? Such questions are of the essence in dealing with anomalous findings. They call for judgments which cannot be made through application of any strict criteria of confirmation or refutation, because such judgments go beyond the available evidence to assess the relevance of the given anomaly in the context of knowledge which has not yet been acquired. They involve an estimation of the consequences of treating the anomaly in one way or another, an estimation which

therefore has the character of a prediction of future knowledge.

Thus, anomalies, like other unsolved problems in science, may provoke intense theoretical and experimental efforts at assimilation, or force ad hoc revisions of theory, or be shelved pending the further development of science, or simply be ignored. Only rarely do they serve to refute any well-attested and ongoing theory with regards to which they are anomalous. Given the ubiquity of anomalous results in scientific investigations, this chariness concerning acceptance of them as refutations is entirely appropriate. Many or most anomalies prove assimilable within further extensions of current theory or through more precise application of theory (the anomalies for Newtonian and Copernican theory cited above were eventually resolved in this latter way; see Kuhn, 1957). Hasty acceptance of anomalies as constituting refutations would thus lead to the rejection of theories or of parts of theories which in some cases would later prove perfectly competent to account for the anomaly, or which in other cases might in the same interim period lead to other new achievements which would render them worth retaining despite the continuing presence of the anomaly. Such a chary attitude towards anomalies is perfectly consistent with a realist orientation to science (if nature is assumed 'really' to be the way the theory says it is, then an experimental result which blatantly contradicts the theory will naturally be looked on with some suspicion), but is clearly not consistent with any rigorous commitment to formal testing procedures.

Second, if in some respects such formal testing procedures are too harsh on a theory, in other respects they are too easy. Any anomaly can be accommodated within a theory by making an ad hoc addition to the theory or by referring the anomaly to some other theory which

must be assumed valid for the theory under examination to be tested. As Feigl (1956, p. 12) regretfully puts it (regretfully, because he is concerned to justify formal criteria as far as possible), "from a purely formal point of view it must be admitted that adjustments in any part of the theoretical network may result in a better empirical 'fit'⁹." Thus, Newtonian theory could have been 'saved' from the anomalous evidence of the moon's orbit by postulating that the moon was of non-uniform density, so that its centre of gravity did not correspond with its physical centre-point. The parameters of the moon's density distribution could have been chosen freely so as to induce just that correction in its predicted orbit that would yield conformance with the observed one. Indeed, Newtonian theory towards the end of its career was saved in just such a way from the results of the Michelson-Morley experiment. The Fitzgerald equations predicted the contraction of a body in the direction of travel to an extent which would precisely counterbalance what would otherwise be the measured effect of the ether drift; the failure to find evidence of an ether drift thus no longer formally told against the theory. This formal rehabilitation of Newtonian theory did not, of course, prevent the Fitzgerald equations from being used by Einstein for quite a different purpose, in the establishment of special relativity theory.

In short, a strict insistence on compatability of theory and observation (of the predictions entailed by the theory) would require that every anomalous result be dealt with when it occurred, but would allow the dealing-with to be ad hoc and trivial. Indeed, such an insistence would force the dealing-with to be ad hoc, since it would rule out any delay, any further gathering of evidence, insight, or interpretation, prior to considering the anomaly if the theory was to

be maintained.

Third, and closely related to the availability of ad hoc defensive procedures in a theory, is the fact that an unequivocal comparison of a theory with all potential alternative theories is never possible. This impossibility is not simply due to the infinity of possible alternatives. Rather, there is always more than one possible theory that will entail a given set of experimental results, account for a given set of events. Alternatives to a given theory, possessing this property of empirical equivalence, may be trivial, as contrived examples usually are. For instance, as an alternative to theory T we could propose theory T + M, where M is a meaningless statement or set of statements sanctioned as empirically significant by any of the inadequate meaning criteria described in the previous section. On the other hand, each alternative may be a highly developed and fruitful theory in its own right. For instance, the relevant parts of the most refined version of Newtonian mechanics were in this sense empirically equivalent to special relativity theory, or could have been rendered so with slight further modifications, at the time special relativity theory was first proposed. As another example, the Copernican and Tychonic astronomical theories were empirically equivalent with respect to predictions concerning events within the solar system--in this case for the rather special reason that they were also geometrically equivalent. (They were not, however, thus equivalent in the context of the universe as a whole, since Copernican theory predicted stellar parallax and Tychonic theory did not; the evidence available at the time supported Tycho.)

Choosing between such empirically equivalent theories cannot, by definition, be done on the basis of the evidence in favour of each.

Empirically equivalent theories can, nevertheless, often be forced to become non-equivalent, by elaborating both to the point where they yield differential predictions. Such a procedure can be effective, however, only with the imposition of stringent limits on the amount of post hoc accommodation that will be permitted to the theories. Such limits, furthermore, cannot themselves be formally specified in any way that will prevent either trivial or possibly significant exceptions, any more easily than meaning criteria can be specified, and for much the same reasons. The imposition of such limits on post hoc accommodation in the absence of formal preset criteria for doing so therefore amounts to placing limits on what will be accepted as attributable to the world. The practice of doing so is thus consistent with the position that what is taken to be the autonomous constitution of the world should exercise a directive influence on the content of scientific theories--in short, with a position of scientific realism--but is not consistent with a position which lacks (and could be taken to abjure) any systematic justification or rationale for deviating from the requirements of unequivocal empirical and logical grounds for the assessment of theories. The same position of scientific realism provides grounds for rejecting a preferred alternative theory which is equivalent only trivially to a given theory, since alternatives of this sort involve the same kind of post hoc accommodation just considered, without benefit even of a prior deciding experiment.

On the other hand, choosing between non-trivially equivalent theories, both of which are significant and well-developed, in the absence of what is accepted beforehand as deciding evidence, is not rendered any systematically easier by adopting a realist approach than by adopting a positivist approach. Nor is there any reason why it

should be easier; the systematic function of realism in the context of construction (as contrasted with its psychological function as emphasized by Planck) is not to provide a basis for making judgments in the absence of evidence, but to provide a basis for making judgments on the basis of evidence which is not logically compelling. When the choice must be made, therefore, between Copernican and Tyconic astronomy, between Newtonian mechanics and special relativity, scientists who make the choice on realist grounds may well disagree in their choices. As will be seen, the circumstances in which such choices are made often serve to mitigate the arbitrariness and 'all or nothing' character of the choice.

It should be mentioned that some of the same considerations which have been brought forward here in support of a realist position--the impossibility of making completely unequivocal tests or crucial experiments to decide between alternative theories--led Poincaré (1905) and Duhem (1914) to adopt a precisely opposite position. They maintained that since scientific theories cannot be unequivocally tested or compared, therefore we cannot assume that they make definite contact with the real world. The truth that is allegedly sought by scientific theories, after all, is presumably univocal, even if the theories themselves are not. Therefore, they maintained, the multiplicity of possible scientific theories dictates that such theories cannot be considered 'true' in anything related to an absolute sense, but should rather be considered as conventions, to be adopted on the basis of their convenience and discarded when they no longer prove convenient.

Such, in brief, is the position associated with the particular form of positivism known as conventionalism (cf. the discussion on pp. 97ff.). It is certainly a consistent position, although it can be

argued that the progressive character of science renders it only trivially so (e.g. Grünbaum, 1966). It does, furthermore, offer one means for resolving the difficulties generated by reliance on formal testing procedures; and it will be seen later that, on the account presented here, it was an appropriate means to employ at the time the position was advanced. There is no reason to accept it as either generally appropriate or logically compelling however. The conventionalist position amounts merely to the demonstration that commitment to formal testing procedures, with the practically limitless ad hoc adjustments to theory which such procedures allow, is incompatible with commitment to a realist account of science. Specifically, the conventionalist position combines commitment to formal testing procedures with a keen appreciation of the fallibility of such procedures, and uses the conjunction to justify withholding commitment from the theories which are sanctioned by such procedures. The conventionalist position effectively repudiates realism, therefore, only if the formal testing procedures are given independent prior affirmation, but conventionalism does not itself provide any grounds for making any such affirmation. Thus, conventionalism does not tell against the realist orientation advocated here, which is in part an alternative means for dealing with the same problems as those to which conventionalism is addressed (resolving them by de-emphasizing the testing criteria rather than by de-emphasizing the theory), but which is also vindicated initially by the incoherence of formal criteria even in advance of their application to the testing of extant theories. Furthermore, the choice between alternative extant theories is not in any case simplified by designating the theories as conventions¹⁰. Such a designation is indeed independent of the problems which gave rise to the conventionalist position, problems

associated with choosing between theories--except in the special case where an out of date but familiar theory is maintained on the basis of its everlasting truth. Such cases belong, as a rule, to the context of reconstruction, and will be considered there.

Realism and Positivism in the Context of Construction: 3.

Problems Arising from the Resort to Methodological rather than Logical Criteria.

The practical task of assessing statements or theories in terms of their empirical content or meaningfulness, and that of determining their validity, are thus equally unresolved so far by the application of strictly or consistently logical analyses. A positivist orientation towards the conduct of science thus cannot, on the available evidence, be coherently implemented as a formal programme. That is, no attempts at implementing it as a formal programme have been successful. There is nothing in what has been said, however, to suggest that positivism cannot be consistently or coherently maintained as an attitude toward the conduct of science, even in the absence of adequate formalizations. Just how much the attitude, in the absence of adequate formalizations, will be able to accomplish, is certainly an open question; but it is not unreasonable to expect that it might have some use. In line with such considerations, some logicians and philosophers of science have insisted that the value of meaning or demarcation criteria is not altogether negated by the logical weaknesses displayed by such criteria. They maintain that the purpose of constructing such criteria--that of ensuring the unequivocally empirical character of science--can effectively be served by applying the best available criteria to the best of our ability. At the very least, it is maintained, such a procedure, while not proof against errors, is better than none at all. Maintaining such

an orientation towards the construction and use of meaning and demarcation criteria characterizes the position as a methodological, rather than a logical one. That is, such a position is concerned with the construction of techniques for achieving a given end, that of ensuring the empirical character of science; the techniques are, accordingly, to be judged primarily on the basis of how well they can or could achieve that end, not on the basis of their logical properties per se. Maxwell, who does not himself subscribe to this position, summarizes it as follows (the letters 'M' and 'P' refer back to the discussion of the confirmability criterion on pp. 247-248; they replace equivalent symbols in Maxwell's text):

"It is true that the criterion will not do as it stands; but what is intended is quite clear. Let us modify it to read something like, 'Some observation sentence must be derivable from M and P, and P must function nontrivially and in an uncontrived manner in the derivation.' It is true," this position continues, "that we may not be able to give formal criteria for triviality and for uncontrivedness--criteria which will meet all of the logician's trick cases. Nevertheless, we know what is meant and will be able to recognize triviality, etc. (in this sense) when we see instances of it" (Maxwell, 1966, pp. 320-321).

This methodological position amounts at times to that of treating the available meaning and demarcation criteria as if they were valid, and adapting them to meet special cases where they might lead to error. However, it is not clear at the outset, at least, how formal criteria can be fully rehabilitated by any such retreat from the requirements of formal coherence. If one liberalizes the rules for the application of formal criteria, does one end up with informal criteria, with unreliable formal criteria, or with nothing at all? That is, the problems with any pragmatic use of such formal criteria concern the identification of the special cases where the criteria are inapplicable, and the decision about what to do with them once they have been

identified. The "logician's trick cases" can be recognized easily enough, if for no other reason than that they are put forward by logicians. But as pointed out before, the value of such trick cases is that they show that a proposed criterion can lead to unacceptable conclusions--and they show it in such a way that the conclusions can easily be identified as unacceptable. No such ease of identification is provided if application of a proposed criterion leads to contentious or problematic conclusions in the analysis or construction of complex theories. There, it is precisely in the difficult situations where sense might be mixed with nonsense and the result prove opaque to common sense inspection that the application of the criterion is most relevant; and it is precisely in those situations that the logical fallibility of the criterion will render its applicability most in doubt. Hence, if a proposed criterion is not valid in all situations, as none of them are, it will have to be supplemented by something else--common sense, commitment to the realistic significance of an explanatory schema, etc.--in just those cases where the criterion alone could, if it were generally adequate, prove most useful. In short, a fallible criterion can be expected to abdicate its responsibility just when it is most needed, when confronted with the most difficult tasks; it will do so not because it is more likely to lead to absurd conclusions when applied to difficult tasks, but because the informal checks to which it must constantly be subject are themselves most in doubt when the tasks are difficult.

Let us apply these considerations to the two most influential methodological positions put forward to date, falsificationism and operationism. Both were initially developed before the search for formal criteria reached its peak, and both remain influential at present, in the face of the reduced influence of formal criteria.

Falsificationism. Falsificationism as a methodological position is associated almost entirely with Popper and his more faithful students. Popper is not and never has been a positivist, in the sense in which that term has been used here. On the contrary, he has always regarded the goal of science as the attainment of absolute truth. He regards this goal as unattainable in principle however, and has devoted most of his philosophical career to the development of logical and empirical decision criteria for the unambiguous assessment of statements and theories¹¹. These criteria are different from, but entirely comparable with, the meaning criteria developed by logical positivists. Popper's appreciation of the fallibility of human knowledge, however, has led him to stress the provisional character and fallibility of any such criteria as tools in the search for knowledge. Against the hopes, at least, of some logical positivists, he declares that the application of any demarcation or testing criteria can never lead to any certainty regarding either the truth or the falsity of any statements: "If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are (Popper, 1959, p. 50).¹²" Since his falsificationist criteria of demarcation and (derivatively) theory-testing are parts of a general methodological position, he is able freely to supplement them when in unadorned form they are insufficient to establish or guarantee the empirical character of science. For instance, in countering the charge that any theory could be made to resist falsification on his criteria, simply by making ad and post hoc adjustments to it as necessary, Popper states: "the empirical method shall be characterized as a method that excludes precisely those ways of evading falsification which, as my imaginary critic rightly insists,

are logically possible (ibid., p. 42)."

How far do such supplementations render falsificationism workable? Three major problems for logical (as contrasted with methodological) falsificationism have been mentioned so far. They are the admission of meaningless statements of the same sort as are sanctioned by the confirmability criterion (p. 248), the 'immunization' of theories against refutation by an injection of ad hoc-ness (p. 255), and the unfalsifiability of existential statements (p. 246). Concerning the first of these, Popper has little to say, trusting presumably (like the fictional methodologist portrayed by Maxwell) that such instances can be recognized when they occur; the dangers inherent in this trust have already been mentioned. Concerning the second, Popper's answer is necessarily complicated by the fact that an addition to theory is not usually identifiable as simply ad hoc or not; there are degrees of ad hoc-ness, corresponding to degrees of empirical content and degrees of increase therein. While the Fitzgerald contraction equations and the associated Lorentzian theory are not completely ad hoc (ibid., p. 83; cf. above, p. 256) they generate fewer testable consequences than does special relativity theory, which, Popper claims, is therefore to be preferred on the basis of its greater empirical content. Such an answer is certainly acceptable in principle, but it only pushes the problem back a step: How does one now go about comparing the revised theory (Newtonian mechanics + Lorentz-Fitzgerald equations) with the new one (special relativity) on the basis of their differential empirical content? Empirical content can be defined as a function of the number of testable predictions entailed by a theory, and, again in principle, two theories might be compared on this basis. A technique for making such comparisons is difficult to develop, however, since sets of

theoretically entailed predictions tend to be, if not empty, infinite in number. Popper sketches the outlines of such a technique, but his account is no more than schematic and broadly descriptive; it is more an analysis of the problem than an attempt at a solution. Neither Popper nor anyone else has yet been able to develop such a technique to the point where it could be applied in any concrete instance. In the absence of such a technique, the placing of restrictions on ad hoc-ness can only be based on the content of the ad hoc additions and their parent theory, considered separately from their number. Such a practice, therefore, cannot be given a general justification, methodological any more than logical, which is separate in any way from the substantive issues related to the particular case. It constitutes, that is, the placing of limitations on what will be accepted as attributable to nature (as indicated above, p. 258,) over and above those which can be justified with reference to the (methodological or logical) decision criteria. Such a practice, when based--as it must be, if it is not to be random--on broadly theoretical considerations, including, perhaps, intuitive ones, typifies a position of scientific realism.

The third problem is that existential statements, whether also containing a universal quantifier or not, are not falsifiable, and hence on strict application of a falsificationist criterion are non-empirical or metaphysical. Many such statements, especially of the latter (mixed) sort, are nonetheless typically allowed in science and appear in scientific theories. Popper maintains his falsificationist criterion strictly only with respect to isolated existential statements. For those that are included in theories he is more lenient, requiring only that they be incorporated in the theory in such a way as to minimize their purely existential import.

...an isolated existential statement is never falsifiable; but if taken in a context with other statements, an existential statement may in some cases add to the empirical content of the whole context; it may enrich the theory to which it belongs, and may add to its degree of falsifiability or testability. In this case, the theoretical system including the existential statement in question is to be described as scientific rather than metaphysical (ibid., p. 70; italics in the original).

Again, Popper's answer is entirely acceptable in principle, but his statement must nevertheless be read with some skepticism. No matter how pragmatic we wish to be about the testing of theories, the question of whether a theory entails falsifiable predictions or not is a logical one. From the conjunction of an existential and a universal statement, no universal statements are entailed except those entailed by the original universal statement alone. New existential statements may be entailed by such a conjunction, but existential statements are not allowable as theoretical predictions, because as such they are not falsifiable. Hence the incorporation of an existential statement in a theory does not increase the falsifiability of the theory; it does not increase the theory's empirical content in the strict sense of generating additional testable (falsifiable) predictions. On the contrary, if the empirical content of a theory is measured as a function of the ratio of theoretical postulates to derivable predictions (using an as yet undeveloped calculus of infinite sets, as mentioned above), the inclusion of any existential statement will always decrease the empirical content of the theory by increasing the ratio. Since all existential statements are on a par in this respect, Popper's emphasis on "some cases" is, strictly speaking, otiose (as well as in fact unsupported by him); the truth consequences of a theory incorporating existential statements are the same whether the existential statements are transcendently metaphysical or (in some sense) empirically meaningful. On grounds of

falsifiability alone, if any existential statements are allowable in a theory, then all are. The falsifiability criterion cannot distinguish between them.

And yet, Popper is undoubtedly correct when he claims that some existential statements, but only some, enrich a theory or add to its empirical content. The example he gives is that of the discovery of the element Hafnium, and the inclusion in chemical theory of existential statements asserting its existence, after some of its properties had been predicted by Bohr on the basis of strictly universal entailments from theory. The discovery and identification of Hafnium as exhibiting these properties enriched chemical theory, extended its range, may have been instrumental in the discovery of further elements, etc. But the empirical content or meaningfulness of existential statements such as that asserting the existence of Hafnium is not such as can be defined or measured by use of the falsifiability criterion, as has just been shown. The empirical content of such statements must be of a sort that can be assessed, if at all, only on the basis of criteria other than that of falsifiability. Existential statements must be judged empirical or non-empirical, meaningful or meaningless, apart from their incorporation in any theory which receives its evaluation of empirical status strictly on the basis of its falsifiability. Such judgments of meaningfulness or lack thereof can certainly be made on theoretical grounds, but not according to Popper's rules, not, that is, by the use of his version of 'empirical method'.

In short, the use of falsification as a methodological rather than a logical tool does not eliminate the difficulties concerning existential statements. Strict (logical) falsificationism bars them all; methodological fiat, if it admits any, admits them all and cannot

--again, on methodological grounds--distinguish or discriminate between them; their evaluation and the choice between them can be made only on substantive grounds independent of the falsificationist methodology. Such grounds, if they are to have any systematic rigour, can only be theoretical grounds, but it follows that they must be occupied in the absence of, or to an extent greater than that warranted by, strict adherence to the canons of logical or methodological justification. The occupation of such grounds, to such an extent, constitutes, again, a position of scientific realism.

In both of the cases considered, the adoption of falsificationism as a methodological rather than a logical position provides a licence, for repudiating ad hoc-ness and for admitting some existential statements, respectively. But it is only one who has bound himself to explicit rules of procedure who needs a licence to deviate from them, and the licence does not carry with it any new rules of procedure for doing so. In the absence of such rules of procedure, making use of the licence entails making a choice that can later be vindicated pragmatically (if it was the 'right' one) but that at the time can be made only on grounds appropriate to a position of scientific realism.

Operationism. An independent technique for empirically establishing the meaning of concepts was developed by Bridgman (1927), under the name of 'operational analysis', usually shortened (to Bridgman's distaste) to 'operationism'. The fundamental tenet of operational analysis is that the meaning of a term or concept is determined by (Bridgman never quite said 'the same as') the operations performed in applying the concept. As Bridgman put it in a later summary of his position:

The fundamental idea back of an operational analysis is... that we do not know the meaning of a concept unless we can

specify the operations which were used by us or our neighbour in applying the concept in any concrete situation (Bridgman, 1951; quoted in Lindsay, 1956, p. 68).

Like the logical positivist analyses described previously, operational analysis was developed as part of a reaction to the overturning of Newtonian mechanics in the modern scientific revolution, in a spirit of 'How could we have been so wrong?' Bridgman's avowed aim in setting forth the principles of operational analysis was to "render unnecessary the services of the unborn Einsteins (Bridgman, 1927, p. 24)" in dramatically recasting our conception of the universe. His principles have been applied, however, more to rendering unnecessary the services of the unborn Watsons, having been much more influential and widely adopted in behaviourist psychology than in physics. Unlike most logical positivist analyses, and like Popper's, operational analysis was never intended as a formal criterion of meaningfulness but rather as a technique, backed by an attitude, for keeping theoretical constructs in close touch with observations. Nevertheless, attempts to generalize it as a set of explicit principles encounter comparable difficulties to those affecting empiricist meaning criteria, as well as some new ones stemming from its practical character. These will be considered in turn.

We can operationally define the 'length' of an object as the reading taken from a yardstick placed against the object, and we can specify the operations involved in measuring length as precisely as we wish. Similarly, we can define the 'brittleness' of an object as the force of a hammer blow necessary to shatter it; or we can stipulate a brittle-not brittle dichotomy by arbitrarily assigning a cut-off point on the continuum of force (of hammer blows). None of this is problematic. Often, however, we need to attribute a property to something without

actually performing the designated operations for defining that property. It would be inconvenient to have to measure the length of a body every time we wish to refer to its length. It would be impractical to establish the brittleness of a body by hitting it with a hammer until it shattered, if we had need of a whole brittle body. It is therefore necessary to phrase the operational definition as a conditional statement. The meaning of "This flask is brittle" is operationally given by the conditional "If I hit this flask with a hammer at the designated force, the flask will shatter." The meaning of "This desk is three feet wide" is given, similarly, by "If I place a yardstick against the side of this desk, the yardstick will yield a measurement of three feet." The use of such conditional statements establishes the designated properties as dispositional ones; the body would display such a property if it were subjected to the specified operation. All operational definitions are of dispositional properties in this technical sense. The validity of a conditional statement that is untested in a given case--hence, the validity of an operational definition, as contrasted with the operations themselves--is dependent on generalization from a test case; the desk was measured yesterday, a sample of flasks from the carton each shattered when hit with the hammer. From this reconstruction of operational definitions, there are two problems which arise, the first from the form of conditional statements, the second from the process of generalization.

The first problem is that the conditional "If X is hit with a hammer, X will shatter" is satisfied by any X that is not hit with a hammer; this problem is a consequence of the logic of conditional statements (which are falsified only if the antecedent is true and the consequent is false). Thus, in the example given, everything that is

not hit with a hammer is operationally defined as brittle, and (by extension) every desk that is not measured is three feet wide. Those consequences are obviously unacceptable, but they follow inevitably from the use of conditional statements, and it is hard to see how such statements can be avoided. Hempel (1956) suggests that the problems resulting from the use of conditional statements generally can be overcome only by relating the dispositional property to universal (theoretical) laws which account for it. As applied to operational analysis, such a procedure would involve deducing the dispositional property from a theoretical account, and hence would subsume operational definitions under the general class of observation statements entailed by a theory. Making operational definitions thus dependent on theory, however, negates the purpose for which they were developed, that of defining a concept or property independent of its function in a theory so as to place limits on its extension within the theory.

This first problem results from what might seem the vagaries of logic, and hence might be dismissed as irrelevant to a practical technique. The second problem, however, leads to much the same conclusions concerning the relation of operational definitions to theories, and does so on eminently practical grounds. Generalization from a test case--which, as we have seen, is necessary if operational definitions are to be used--involves an inductive risk; that is, it requires the judgment that a given case is similar to the test case with regard to the property considered. One must assume that the desk did not change its width overnight, or that the quality control at the glass-blowing works is sufficient to guarantee that each flask in the carton is equally likely to break. The fact that these assumptions can be made only within certain limits is not a concern in these cases. We

can safely make the necessary assumptions in the cases cited, because we are familiar with the way in which desks and flasks generally behave. That is, we know what variables can for our present purposes be considered irrelevant, can be left out of consideration, in attributing the same property to two different objects or to one object at two different times. More precisely, we can and must judge that the operationally defined value of a designated variable is invariant with respect to changes in the value of certain other variables, in order for our operational definitions to have even the slightest degree of generality. We assume, for instance, that desk-width (as operationally defined) is invariant with respect to time, or that flask-brittleness (as operationally defined) is invariant with respect to denumeration.

Thus, from the necessity of inductive risk we are led to the necessity of assuming invariance of certain variables with respect to others¹³. These assumed invariances establish the limits within which operational definitions can be considered valid. They are typically not specified in reports of experimental procedure because they are, in fact, infinite in number, comprising all of the unique circumstances under which the operational definition was made. The only variables with respect to which invariance is not assumed are those specified in the operational definition itself. Thus, the operational definition of 'intelligence' as 'the score achieved on an intelligence test' assumes invariance with respect, among other things, to choice of test. The operational definition of 'intelligence' as 'the average score achieved on the verbal and performance subscales of the Stanford-Binet Intelligence Test, 1954 revised edition, administered by a trained tester and in conformance with the test instructions printed in the

test handbook' assumes invariance with respect, among other things, to sex, age, and social status of the tester.

Thus, operational definitions are always definitions with respect to certain variables and without respect to all others, which are presumed irrelevant. The operationally defined concept, that is, is presumed variable with respect to the implicated variables and invariant with respect to the rest. The assumption of invariance can be justified on, at best, two bases. First is informal familiarity with the objects and properties involved; there is no reason to expect that desk-width varies with time because it has never happened before and besides, desks don't do that sort of thing. Second is a theoretical account that specifies the behaviour appropriate to the objects and properties involved; the definition of a solid in conjunction with the heat expansion coefficients of steel and wood, for instance, may enable us to predict, or may be taken to guarantee, that the desk will not change its width overnight. The assumption of invariance (or irrelevance) is not rigid of course; it can be tested with respect to any variables chosen for consideration and available for measurement. The choice of variables to test in this way may be made on almost any basis whatever--common sense, hunches, random selection, theoretical expectations--but is not implied in any way by the procedures of operational definition itself, and can never be exhaustive.

Let us apply these considerations to the operational analysis of the concepts of Newtonian theory. In that context, Bridgman's claim was that careful operational definition of concepts such as mass, distance, simultaneity, etc., could have prevented physicists from assuming instantaneous action at a distance and similar counterfactuals generated by Newtonian theory¹⁴. However, while it is perfectly true

that operational analysis of these concepts, with respect to the variables and limits later emphasized by relativity theory, could have had such a limiting effect on physicists' expectations, the point is that there was no reason--and no way--for the analysis to be carried out in that way at that time. What we would have to recognize as operational analyses of a very sophisticated sort were carried out on all these dubious concepts--mass, force, distance, simultaneity, etc.--quite regularly in Newtonian physics. But just as, to make generalization from specific observed instances possible, we assume that width is invariant with respect to time, so did Newtonian physicists assume that length and mass were invariant with respect to velocity, time determinations with respect to distance, etc.--they had no reason to assume otherwise, and they had a theory that assured them that their assumptions were tenable. Nor were any reasons available to them to question their assumptions, on the basis of any measurements which they could make. Carrying out operational analyses with respect to the limits emphasized by relativity theory would not, after all, have exposed any glaring counterfactuals in Newtonian theory; nor would it today. Such an analysis would not have disclosed the dependence of mass on velocity or of time determinations on distance. The discovery of these relationships required--among many other things, including a directed search--measuring instruments more precise than any available before the last third of the nineteenth century. Thus, a relativity-inspired operational analysis conducted before that time could have revealed only that, within the limits of measurement, the assumed invariances held. Refusal to generalize beyond the limits of measurement could only have restricted the application of theoretical concepts in a way that both then and now would seem gratuitous. Any analysis,

for instance, that could have forestalled the assumption of the existence of signals or forces with velocities greater than that of light (which, for Newtonian theory, was a purely arbitrary limit) could equally have been applied to exclude consideration of any material bodies with velocities greater than, say, half that of light. In each case, that is, the sole basis for refusing to assume that such velocities occurred would have been that none such had ever been observed. We now have good reasons for accepting the first limit and not the second, but no such reasons were available to physicists until near the end of the nineteenth century. In the absence of a theoretical basis for distinguishing between the two limits--signals at light velocity and bodies at half light velocity--they could not have been ascribed any differential significance. Finally, it should be mentioned that any such restriction of theoretical concepts on the basis of the limits emphasized by relativity theory might well have been, not merely gratuitous in Newtonian theory, but utterly disruptive of it. The objectionable aspects of the concepts of mass, distance, simultaneity, etc., were not ancillary to Newtonian theory but fundamental to it. Grünbaum (1956) has argued, therefore, that any analysis which could have precluded the assumptions of absolute simultaneity and instantaneous action at a distance would have prevented the formulation of the theory from the beginning.

In short, operational analysis can delimit the application of a concept only along those dimensions where its propriety is already suspect¹⁵. Use of the technique to delimit concepts in any other way would necessarily rest on a choice of variables or dimensions random with respect to the theory in which the concepts appear; such use would be, in the context of the development of the theory, gratuitous at best¹⁶

and fatal to the progress of science at worst. In between, it may be merely trivializing. The choice of variables to control for--that is, the selection of suspect dimensions--cannot be made on the basis of the principles of operational analysis itself; such variables are not specifiable, even as regards their general type, independent of the theory and the problems with which they are connected. If the choice of such variables is to be systematic, it must rest on broadly theoretical considerations, but in the absence of a finished theory that entails the relevance of specified variables--that is, in the construction and development of a theory--the theory-based choice must proceed on grounds other than, or weaker than, entailment. Again, the assumption that nature autonomously possesses the characteristics, or some of them, attributed to it by the theory, that is, the attribution to the theory of greater validity than is strictly warrantable, can provide such grounds; and again, the holding of such an assumption constitutes a position of scientific realism. Thus, operational analysis, like methodological falsificationism, requires for its systematic utilization in the context of a developing theory a position of or equivalent to scientific realism.

Realism and Positivism in the Context of Construction: 4.

Conclusion.

This concludes the critique of positivism, and its contrast with realism, in the context of construction. We may summarize the discussion to this point by saying that theory-evaluations and other scientific decisions, when based on explicit criteria either of a strictly logical or of a methodological type, are often equivocal at best and incoherent at worst. That is, the criteria do not in fact apply unambiguously to all the situations which require decisions of

a sort to which the criteria are supposed to be relevant, decisions concerning the empirical meaningfulness and validity of concepts and statements. In such cases, the proposed criteria for evaluation stand in need of replacement or supplementation with an evidentially unwarranted and strictly unwarrantable commitment to the unqualified truth or falsity of a scientific theory, in order to provide a systematic basis for the making of such decisions. We now turn to the other side of the story, the comparison of positivism and realism in the context of reconstruction. The exposition in this case will be based mainly on what has already been said, and so can be considerably shorter than in the previous discussion.

Positivism and Realism in the Context of Reconstruction.

To return to a point which was made before, the systematic or pragmatic justification for realism in the context of construction is that it provides a basis for making decisions or arriving at conclusions on the strength of evidence which is not logically compelling. Therein lies the strength of realism, and also its weakness: it enables decisions to be made, thereby preventing intellectual paralysis, but by the same token may encourage them to be made wrongly. Drawing conclusions on the basis of non-compelling or, strictly speaking, insufficient evidence necessitates a relaxation or abridgment of critical standards. Such standards are in part replaced by reliance on the power of a theory, or of the fundamental insights which the theory exemplifies, to serve as a guide in the continuing investigation of nature. The reliance on the theory's power is, if not altogether uncritical, at least relatively so; it must be strong enough to warrant ignoring or minimizing the claims of the flood of anomalies that besets any theory, as well as those of the innumerable alternative theories that are always logically possible.

A theory that is maintained in such a way is not, of course, necessarily monolithic; the realist commitment is not to the theory in all its details but to what are taken to be its fundamental insights. Thus, the theory can be revised, modified, extended, and improved on the basis of applying these insights to the solution of further problems. The fact of commitment, however, places certain stringent limitations on the direction and extent of possible theoretical development. In particular, it makes it very difficult for the theory, or for the scientists who maintain it, to incorporate or adopt any interpretation of observed events which contradicts the fundamental insights of the accepted theory, any interpretation, in other words, which is incompatible with (what the theory claims to be, hence what is accepted as) the real world. If the investigation of nature by means of an accepted theory leads to a situation in which such 'unreal' interpretations are called for or, more accurately, in which the order and arrangement that nature displays in conformance with the accepted theory begin to break down and can be restored only by the adoption of such 'unreal' interpretations, then the realist approach to science runs into very serious trouble.

Such a situation can arise when experimental findings, or conceptual analyses thereof, that are incompatible with the fundamental insights of the accepted theory come to the fore. They may come to the fore, and thus be separated from the perennial flood of unresolved theoretical and experimental anomalies, in a variety of ways. They may arise as counterfactuals to theoretical predictions which are of crucial importance to the accepted theory by virtue of relating closely to the theory's most fundamental insights or precepts; the recognition of such counterfactuals as particularly important thus proceeds, in this case, from the very fact of the realist interpretation of the theory. They

may arise as a result of external, non-scientific criteria of relevance, such as the demands for better navigational aids, calendars, or intelligence tests; such demands can confer upon some unresolved theoretical problems a more intense focus than they might have received on autonomously scientific grounds. They may arise from the success of a competing theory, with a different set of fundamental insights or precepts, in predicting and accounting for an overlapping set of events, especially if the success of the competing theory thereby attests to the power of the different set of fundamental insights which it exemplifies.

Although such anomalous findings and interpretations may arise from many sources, and be ascribed importance for as many reasons, it is the commitment to realism that renders them of telling importance to the particular accepted theory. Once it has been agreed that they must be dealt with in the course of science, that is, that they can be neither denied or ignored, the failure in fact to deal with them within the accepted theory--if, indeed, it continues to be a failure, if the new intensity of focus upon them does not lead to their resolution--places a realist interpretation of that theory in a double-bind situation. The commitment to the truth of the scientific system with regards to which they are anomalous serves simultaneously to accord them major systematic status and to preclude the employment of any easy stratagems (such as ad hoc modifications to the theory) for dealing with them.

Thus, howsoever such problems arise, they constitute a challenge to the accepted theory of a sort that cannot be ignored but that also, within the constraints of a realist interpretation of the theory, cannot be met. Such unresolvable problems that, for whatever reason, cannot be bypassed or ignored, have a devastating effect upon

the tenets of scientific realism. In the presence of very many of them, the realist conviction that science as exemplified by the accepted theory can disclose the truth about the real world may begin to crumble; alternately, the conviction may be upheld but begin to be divorced from its formally inadequate (and now also systematically inadequate) empirical foundations. Either reaction involves a re-examination of the relationship of the theory qua theory to its empirical base, in order to understand how the two came to diverge. That is, either reaction (and they are not that much different) requires a consideration of the theory and its component parts primarily as abstract constructions rather than as representations of the world. Either, therefore, establishes the context of reconstruction as central to the continuance of the scientific enterprise.

In short, as indicated in the initial description of the two contexts in Chapter 3, the context of reconstruction emerges as primary when scientific focus must be significantly shifted from that part of the world which the theory addresses, to the theory itself. There are three tasks for scientific inquiry in the context of reconstruction. First is the determination of why it is that the accepted (or, for some, formerly accepted) theory is no longer proving adequate to the demands made of it. This task involves identifying the factors within the accepted theory which have come systematically to (as it now seems) vitiate it. Such factors may include the assumptions involved in the theory's formulation or application, the choice of variables and entities in terms of which the theoretical explanation is given, etc.; some or all of these may be identified as empirically unwarranted, overextended, or simply meaningless. Second is the correction or elimination of these vitiating factors, either by recasting them in an

empirically more warranted form or by replacing them with altogether new ones; in the former case the appearance, at least, of the old (i.e., accepted or formerly accepted) theory may be preserved. Third is the finding of solutions to the intractable problems that precipitated the reassessment in the first place. The successful completion of these three tasks imposes demands, both of a technical and of an attitudinal character, which can be met much more easily within a positivist orientation to science than within a realist one.

The main consideration that makes a realist approach unsuitable when applied to these tasks is the comparative inflexibility of a realist approach in dealing with substantive issues. A realist orientation facilitates an intensification of focus on what is known or taken to be important; it is thus well suited to highly directed activities such as adding new findings onto old frameworks, expanding a theory built on an established base, or rejecting one of a pair of incompatible theories. This intensification of focus exacts a price, however, in that it implies a comparative blindness outside the area of focus; the restricted and comparatively linear mode of assessment characteristic of a realist approach renders it incapable of supporting a critical analysis of all components of an accepted theory equally, since some fundamental components will be accorded status which is almost unquestionable, or at least less questionable than others as a function of how fundamental they are. If some of these fundamental components of a theory are precisely the ones identified as responsible for many of the difficulties encountered by the theory, a realist interpretation of the theory may actually hinder its critical analysis. It is under such circumstances, as suggested earlier, that tenacity in the defence of a theory may degenerate to dogmatism: the attitude does

not change, but its degree of appropriateness does.

A realist approach, even if not tied to the old theory, may also hinder the development of solutions to the crucial problems which brought the old theory into comparative disrepute. The realism-derived requirement of systematic coherence, that is, of the compatibility of different solutions to different problems, may impose constraints on the solutions of specific problems which serve only to make the problems even more difficult. In addition, some of the basic tenets of the old theory may have become enshrined as common sense, either throughout the culture or, in more limited form, for those scientists who have been working with the old theory. Even in the absence of a specific systematic commitment to the old theory, such common sense constraints on what can be accepted as attributable to nature may promote prejudgment of the validity of counter-intuitive solutions to outstanding problems.

A positivist orientation, on the other hand, involves no commitment to the truth or ontological significance of a theory, but only to its empirical foundations. Such an orientation thereby facilitates the performance of the tasks appropriate to the context of reconstruction, both in general and in specific ways.

The general facilitative effect of a positivist orientation in this context is a simple consequence of its abjuration of commitment, of its refusal to agree that a theory must be either true or false; that is, it reduces the stakes in the evaluation and selection of theories. Positivist analyses of the status of scientific theories merely as classifications of observed data, or as freely chosen conventions, or as strictly empirical knowledge-claims in which ontological references are contaminations, all serve to reduce the tenacity with which an old

theory is maintained by virtue of devaluing the status of all theories; or at least, they do so for those who accept the analyses. Such analyses may prove attractive, furthermore, for those who cannot cast off the old theory but who acknowledge the cogency of the empirically based objections to it. By divorcing the absolute truth of a theory from its evidential support, positivist analyses enable a discredited but familiar and well-loved theory to be elevated covertly to the status of a metaphysic; it can be maintained as true, and any new ones considered merely as aids to calculation. In this way, the insistence on the truth of a well-known but increasingly untenable theory can be rendered harmless in the course of science, and even what was previously seen as a limitation on the applicability of positivist analyses can serve a useful function. Finally, such positivist analyses can serve also to reduce the diffidence associated with proposing a new theory, minimizing such considerations as that a new theory is either grossly counter-intuitive or contradictory of already existing theories. It was this last feature of positivism, its encouragement through conventionalist analyses of free flights of creative imagination, that was most emphasized by Poincaré (1905).

With regard to the specific tasks necessary in the context of reconstruction: in the identification and analysis of disruptive factors that have vitiated an accepted theory, a positivist orientation contributes, first, an intellectual climate in which the task can be freely undertaken (as just described), and second, the beginning, at least, of a set of analytical techniques appropriate to the task. These techniques stem from the basic positivist principle of requiring the terms and statements of a theory to be free of ontological presuppositions, or to have unambiguous empirical content, and thus include all the

techniques developed for implementing this principle. That is, they include all the recent meaning and demarcation criteria--in their application as methodological techniques--that have been critically examined in relation to the context of construction. They were not appropriate or sufficient in that context, even when considered from a methodological rather than a rigorously logical standpoint, principally because the development and elaboration of theories required the making of decisions concerning the admission and interpretation of observed data to which these criteria were not relevant. That is, the implementation of a positivist orientation through the use of these techniques did not provide answers to many of the most important questions that arose in their supposed field of application, relating to the meaningfulness, empirical content, and validity of selected terms, concepts, and statements. In the present context however, that of reconstruction, and particularly with regard to the present task of identifying and analysing the disruptive elements of a theory, the relevant questions concern precisely the unambiguous empirical content of selected concepts and statements in the theory. The kind of meaningfulness and validity that is to be ascertained and designated as trouble-free is precisely that restricted but unequivocally empirical kind that can be reliably ascertained through use of the techniques. Any surplus meaning attributed to the concepts and statements in the theory is legitimately suspect, in the present case. True, as we have seen it is typically necessary in developing a theory to extend theoretical concepts beyond the range of their unambiguous empirical warrant, and thus to invest them with surplus meaning; but certain instances in which they are so overextended are the ones that lead to trouble for the theory, and these require identification and analysis.

The extent and direction to which any chosen concepts are thus overextended can be determined by application of the positivist methodological techniques. It was, for instance, through analysis of the possible observational implications of the current theoretical conceptions of space and time that Mach was led to conclude that the concepts of absolute space and time were meaningless. Such an analysis of theoretical concepts can be carried out at any time of course, and the positivist methodological techniques are always the appropriate ones for doing so; however, since, again, theoretical concepts are in this sense quite typically overextended, the use of the techniques is called for only in the comparatively rare situations in which such overextension can be identified as responsible for a breakdown in the theory.

The correction or elimination of the disruptive factors in a theory follows on their identification and analysis. In some cases the concepts or assumptions selected for examination may be concluded to be incorrigible or meaningless, and in need of complete replacement. However, if the selected concepts have any ascertainable empirical content, they can be recast so as not to transcend it, by having their application restricted to that which follows from their definition by means of the analytical techniques, e.g., from their operation definitions¹⁷ or from the statement of their confirming or falsifying instances. Such restriction on the applicability of concepts renders them of less power in any proposed new theory of course, but in a sense prepares them for the investment of surplus meaning of a new sort in future theories by stripping them of the particular, no longer acceptable, surplus meaning which they carried in the old theory. The recasting of the offending components of the old theory, when added to the general positivist absence of commitment, facilitates the extraction from that theory of

those parts of it or those applications of it which can still be considered workable. Such extraction is necessary; the old theory may still serve as a useful guide in some scientific investigations, and may have many practical applications as well. It could not be maintained as true on a realist interpretation, because the exceptions to it are too great, but to reject it altogether as false would, in the absence of an alternative theory of comparable power and scope, signal an unacceptable loss of economy. On a positivist interpretation the theory can be maintained in whatever form can be empirically justified; in such a form the theory may have lost much of its elegance and systematic unity, but at least its utility is preserved while it is prevented from acting as a straitjacket on the development of alternative, possibly incompatible theories and models.

Such alternative theories and models may be directed primarily at resolving the outstanding theoretical and experimental problems that touched off the reappraisal of the old theory. The solution of such problems comprises the major substantive achievements made in the context of reconstruction. In the attack on such problems, a positivist approach encourages a conceptual flexibility that permits the tentative pursuit of diverse and even incompatible routes towards their solution; again, this is the feature of positivism emphasized by Poincaré. In the attack on such problems, a positivist orientation, or rather the absence of the complementary realist orientation, serves also to diminish the force of any systematic requirements, such as those of generality and simplicity, in the evaluation of a proposed solution. Such systematic considerations are often legitimately relevant to the evaluation of problem-solutions, but not when applied to the kind of intractable problems considered here, which were implicated in the breakdown of a

previous systematization. Such problems are likely to be hard enough to solve by any means, without imposing requirements on their solutions concerning their applicability to other problems throughout the field of inquiry. Finally, a positivist orientation contributes an emphasis on conceptual precision and rigorous determination of data that draws attention to the fine structure of the observed experimental phenomena, and may thereby be of considerable instrumental utility in focusing attention on previously neglected or unconsidered aspects of the problems. In short, in the solution of outstanding problems a positivist orientation promotes a narrowing and intensification of focus on the phenomena related to the problems and does so without a corresponding systematic commitment. The combination of intensity of focus and conceptual flexibility would, indeed, provide an ideal orientation for the practice of science, if science were not also concerned with the construction of theories possessing maximum scope and generality.

In speaking of the appropriateness of positivism to the context of reconstruction it may be valuable to distinguish, not only between its application to different tasks, but also and more generally between its critical and its mediating function. The critical function of positivism is exemplified by the analysis of overextended or otherwise unsound theoretical concepts, and by the recasting of such concepts, where possible, in such a way as to afford them more solid empirical anchorage; this function of positivism has already been discussed in sufficient detail. The mediating function of positivism is exemplified by its analysis of the status of theories as conventions, classifications of data, etc. By reducing the stakes involved in the assessment and comparison of theories, a positivist orientation to science mediates between successive dominant theories or sets of theories in a branch of

science, easing the transition from one to the next. It is able to do so by being available for use simultaneously as a conservative and as a radical influence--'conservative' and 'radical' meaning nothing more here than the favouring of 'old and entrenched' vs. 'new and unfamiliar' ideas and theories. The influence of positivism is a conservative one, in that it permits the retention of old theories, or old views about the nature of reality, in the face of new and--from a realist standpoint--disconfirmatory evidence. At the same time its influence is a radical one, in that it facilitates the development of new ideas and theories with a minimum of opposition predicated on their 'outlandishness' or counter-intuitiveness, and stemming from the inertia attained by the ideas associated with the old system. In modern physics, the conservative influence of positivism may be exemplified by the 'saving' of Newtonian theory by the Lorentz-Fitzgerald equations when they were first proposed; the radical influence may be exemplified by Einstein's use of the equations in the construction of special relativity theory. Thus, different scientists, depending on their preferences, can look upon either the old or the new theories as either absolutely (and hence non-empirically) true, or as mere aids to calculation, or indeed as both, so long as the two forms of validity are not confused; separating the two kinds of assessment of scientific theories enables the old and the new theories to co-exist as long as proves necessary.

Co-existence is only necessary, however, so long as the old theory retains its vigorous proponents, and until one or more of the new theories has been developed to the point of being able to replace the old theory, either by accounting for most of its successes (as well as some of its failures) or by successfully devaluing them in favour of a new set of achievements. Specifically, it is in the achievement of

solutions to or resolutions of the outstanding problems that led to the decline and reappraisal of the old theory, and in the extension and systematization of such solutions, that the context of reconstruction gradually merges into and is eventually replaced by the context of construction. Once the critical problems have been solved, or shown to be pseudo-problems and replaced with others that are more amenable to solution, it becomes desirable in the course of science to maximize the range of applicability of their solutions so as to make them the basis for a theoretical account of as much of the field of inquiry as is possible. One can give psychological accounts for this tendency towards theoretical development and resulting systematization. One might appeal to an inherent striving, rooted in the psychology of the creative process, for scientists to see the greatest possible simplicity and elegance in nature; Planck's defence of realism was founded in part on such an appeal. Alternatively, one might appeal to a less sublime striving, rooted in the sociology of the scientific community, for scientists to increase their professional status by maximizing the range of a theory with which they have become associated. Such psychologicistic accounts undoubtedly have their place, particularly in characterizing the scientific conduct of individual scientists, and it is fortunate that the two suggested rationales for theoretical expansion are compatible. The shift from reconstruction to construction can be accounted for on less personalistic grounds, however. Maximizing the range of a restricted theory, and unifying several such which have been developed autonomously to account for different specific problematic findings, makes it possible unambiguously to predict and account for new findings, and to incorporate more of the range of phenomena hitherto explicable by the old theory alone. And on any account of the nature and function of science--whether science

is supposed to be seeking truth or classifying data--it is considered desirable to maximize the range of phenomena for which an account can be given and to eliminate any potential contradictions in explanation. Such systematic considerations may be less powerful in a positivist than in a realist approach to science, and may be more fully subordinated to stringently empirical considerations, but are by no means repudiated. Approaching this goal of systematization constitutes the function of science in the context of construction, and in its pursuit the new theory or theories may be expected gradually to modify or abandon altogether their positivist cast in response to the exigencies associated with their development. The replacement of positivism with realism may be a slow process at best, tied to the entry into the scientific field of younger scientists who are not restricted in their options, as some of their elders may be, either to a positivist renunciation of the search for truth or to an outdated common sense commitment to the truth of the old theory¹⁸. With time, nevertheless, if a new systematization is to achieve maximum scope and power, it is essential, as has been shown, that it come to be accepted as having genuine reference to real things.

By way of summary, it may be reiterated that the characteristics of realism and of positivism are the same, whether they are manifested in the context of construction or in that of reconstruction. The primary characteristic of realism is the commitment to the truth of a scientific system or theory (or possibly to the falsity of a competing one) to an extent greater than is strictly warrantable on the basis of empirical evidence and the rules of whatever logic has been adopted; realism thus encourages systematization at the expense of rigorous logical and empirical analysis. The primary characteristic of positivism is the refusal to entertain any such commitment, and the insistence on

evaluating theories and statements strictly on the basis of explicit logical and empirical criteria; positivism thus encourages rigorous logical and empirical analysis at the expense--largely for technical reasons--of systematization. In the shift from the context of construction to the context of reconstruction and back, what changes, because of the changed circumstances, is the relative appropriateness of these two contrasting orientations to science. In the context of construction realism is most appropriate, since what is required is the systematic development and elaboration of theories. In the context of reconstruction, positivism is most appropriate, since what is required is the critical examination, analysis, and occasional dismemberment of theories.

The Context of Discovery and the Context of Justification.

A corollary of the analysis of the relationship between realism and positivism as developed here is that the hoary distinction between the context of discovery and the context of justification is fully tenable only within what has been called here the context of reconstruction. The purpose of the distinction between discovery and justification is to separate the factors properly relevant to the development of a theory from those properly relevant to its assessment. The context of discovery is now typically considered to be relatively free of rules of procedure; or at least, such rules are considered only as heuristics, with no normative significance. (Herschel, who gave the distinction its first clear modern formulation--although he did not name it--was not so cavalier about the function of rules in the context of discovery, but the rules which he formulated were strict inductivist ones of a sort no longer considered tenable.) The context of justification, on the other hand, is characterized by the application of clear-cut

rules and criteria of assessment of a proposed theory.

In the context of construction, however, it is necessary to go beyond the application of any such clear-cut rules because, as we have seen, such rules are not always applicable. Such rules constitute decision procedures that are applicable to some decisions relating to the evaluation of theories, but not to all. Others require recourse to guesses, intuitions, hunches, etc.--and the 'etc.' specifically refers to decision-making procedures which are systematic but not formalizable, procedures based on commitment to the truth of a theory to an extent greater than is strictly warranted by the available evidence--in order for the decisions to be made at all.

Such limitations on the applicability of decision procedures do not invariably weaken the distinction between discovery and justification. Certainly, considerations relating to the construction or development of a theory can be separated from those relating to its assessment in extreme cases such as that of Kekulé, whose dreams of snakes swallowing their tails suggested to him the structure of benzene rings (Koestler, 1964). In such cases, however, the development of the theory is also divorced from previous theoretical conceptions; and it is specifically in the relationship of new theories to old ones on which they are based, or of which they form extensions, that the distinction between the contexts of discovery and justification breaks down.

When the extreme case of a Kekulé is not involved, and a proposed new theoretical interpretation forms an extension of and addition to an already functioning and accepted theory--as is typical in the context of construction--then the criteria for the assessment of the proposed extension are partly derived from the prior content of the

already functioning theory. The criteria are in part derived, that is, from what the theory is taken to have established as the range of acceptable interpretations of (i.e., possible characteristics of) nature. The already functioning theory can serve as the basis for assessment of its extension in two ways. First, the compatibility of the extension with its parent theory may form one of the explicit criteria for judging it. Second, and more important, in the attempts at independent empirical testing of the proposed extension, the interpretation and judged relevance of the observed data will be, again, partly based on what the parent theory is understood to claim is true of the world. Interpretation of the data will be thus partly determined by the content of the parent theory, not because of any kind of scientific closed-mindedness but because, as we have seen, such interpretation cannot be based on anything else and still be both systematic and unequivocal. For the interpretation of empirical data to be systematic, it must be theory-based; for it to be unequivocal, it must be extralogical (cf. the discussion of methodological falsificationism and operationism, above).

The breakdown of the independent status of the criteria of assessment is sufficient to jeopardize the distinction between the contexts of discovery and justification, because maintaining the distinction depends on the possibility of independent specification of the procedures used in justification. But furthermore, this same lack of independence demonstrates the interpenetration of the two contexts, since it is the same factors--the systematic contents of the parent theory--that are implicated both in the development and in the assessment of the extension of that parent theory.

This interpenetration of the contexts of discovery and

justification, and consequent blurring (at least) of the distinction between them, is due at root to the problems involved in the empirical specification of meaning, that is, in the development of criteria of meaning, demarcation, and, derivatively, testing. It is because such criteria are not fully applicable to the evaluation of theories that theories must instead be evaluated partly on the basis of internally derived contentual and systematic considerations, and it is because theories must be evaluated in this way that the context of justification cannot be either specifically characterized or even unambiguously separated from the context of discovery.

To put it another way, the distinction between allowable and un-allowable, necessary and gratuitous concepts and statements in science (in the development and elaboration of theories, hence in the context of construction) does not exactly correspond to the distinction between meaningful and meaningless, scientific and metaphysical concepts and statements as characterized by the application of explicit meaning and demarcation criteria. If these two distinctions were to correspond, then the superordinate distinction between the contexts of discovery and justification would be both appropriate and necessary. In particular it would be necessary; because for the testing criteria to be applied and the results of such testing accorded any confidence, it is essential that the assessment of the theory (through the statements which it entails) be based on application of the criteria and nothing else.

In the context of reconstruction just such a situation obtains, in which the meaningfulness and validity that is to be allowed to the concepts and statements chosen for examination is precisely that which can be warranted by the use of the meaning and demarcation criteria considered as methodological techniques. In that context, as a result,

the distinction between discovery and justification is essential. For instance, the considerations---whether theoretical, skeptical, or whatever---that led Mach critically to re-examine the concepts of space and time in Newtonian theory are of no relevance to the examination itself or to its subsequent application, both of which are based entirely on the adoption and use of explicit criteria of observational significance. It follows also that the choice of theoretical concepts to examine in such a way is likewise irrelevant to the significance of the outcome of the examination. As in the context of construction (as detailed in the discussion of operational analysis) the choice of concepts to be analysed and the dimensions along which the analysis is to proceed do not follow from the principles of the analysis but must instead be based on external considerations; but in this context, the external considerations are in no way implicated in the substance or effect of the analysis itself.

In short, the distinction between the context of discovery and the context of justification presupposes the tenability and assumes the value of a consistent and rigorous positivist orientation to science; and such an orientation is both tenable and valuable only in the context of reconstruction.

A Note on Origins.

The account sketched here of the relationship of realism and positivism in the conduct of science has close affinities with a number of other positions advanced by philosophers and historians of science at various times. These affinities, some of which are fairly obvious, have not been mentioned so far in order to keep the exposition as simple as possible, but they require acknowledgment at least. The 'fundamental insights' exemplified by a theory and resistant to

modification are similar to the 'metaphysical core' of a research programme as described by Lakatos (1970). The 'systematic considerations' relevant to the assessment of a theory in the context of construction are much the same as those that Margenau (1950) has called 'metaphysical requirements'. The necessity for commitment to the realistic significance of a theory has been discussed in great and perspicacious detail by Polanyi (1958), and there are similarities both in content and in the choice of descriptive terms between his account and the one given here. The conception of science as embodying in part a cyclic character, alternating between periods of progressive theoretical and experimental elaboration of an accepted system and periods of comparative theoretical anarchy leading to replacement or abandonment of the once accepted system, is most closely associated today with Kuhn (1962), and in many respects the present account is most closely related to his¹⁹. Each of these formulations has, inevitably, exercised a major formative influence on the account given here; in turn, the present account is intended as far as possible to complement, rather than compete with, each of these²⁰.

The immediate thematic background to the present account, however, is none of these, but rather the historical and philosophical analyses specifically of the relationship between realism and positivism as made by Meyerson (1930) and Koyré (e.g., 1956). Meyerson and Koyré both characterized realism and positivism in much the same way that those orientations have been described here, and analysed the relations between them with detailed references to the history of science. The present account follows in large part directly on their analyses, but--whatever its other deficiencies relative to theirs--attempts to go beyond them in two ways.

First, Meyerson and Koyré both saw positivism as a kind of enemy to creative scientific thought--in much the same way that Planck did--and took it as one of their major responsibilities to combat it. For Meyerson, positivism was a kind of occasional aberration; his lengthy analysis of the history of science attempted to show that despite the claims of a few individuals such as Mach, a positivist orientation has never been characteristic of true science. Koyré moved beyond Meyerson to the extent of considering positivism a recurrent and cyclical phenomenon in the history of science; but he characterized it entirely as a "phase of renouncement", a failure of nerve that periodically besets scientists in the face of new and unfamiliar concepts. From Koyré's position it is a small but vital step to the position taken here, that the cyclical alternation of positivism and realism is necessary to the continuing development of science, as a result of the differential strengths and weaknesses of each.

Second, Meyerson's and Koyré's accounts were both excessively psychologistic, in that each attempted to ground the tendency towards scientific realism solely in the psychology of the creative process. Meyerson declared that his analysis was really an attempt to discover the fundamental laws of thought by analysing their properties as revealed in their most exalted products, completed scientific theories. Koyré was again less extreme, but after categorizing positivism as a "phase of renouncement" declared that the re-emergence of realism could be accounted for sufficiently by the fact "that by nature there is in man the desire not only to know but to understand (Koyré, 1956, p. 203)." The analysis sketched here and in Chapter 3, by contrast, while not entirely abjuring psychologism, eschews it as far as possible and tries

to account for the necessity, at least, of a realist commitment in the context of construction by detailing the inadequacy of the explicit decision procedures available in its absence.

III. Behaviourism and its Positivism.

Finally, we can return to a specific concern with behaviourism. At the end of Chapter 2 an explanation was promised as to why neobehaviourists were wrong in their shared conviction that "commitment to the procedures of science" and possession of "a set of decision procedures, appropriate to all sciences indifferently" would be sufficient to guarantee the viability and successfully progressive character of their scientific enterprise. The explanation has been some time in coming, for it required an analysis of the historical foundations of behaviourism as well as a schematic overview of the relationship between the methodological and the substantive features of science in general. The explanation has thus been building since the end of Chapter 2, but the specific analytical material on which it depends has been concentrated in the preceding pages of the present chapter.

How Adherence to Explicit Decision Procedures Results in Theoretical Fragmentation.

Contrary to the neobehaviourists' expectations, adherence to the logical principles of scientific methodology can guarantee neither the progressive theoretical development of a scientific field as a whole, nor theoretical convergence of competing positions within that field. Rather, such adherence, when widely accepted as constituting what is essential to the scientific enterprise, can prevent progressive theoretical development and theoretical convergence. Adherence to the methodological principles had this effect on neobehaviourism in two complementary ways, expressed through the relative superordination

of methodological principles and through the relative subordination of substantive ones.

The potentially fragmenting and hence 'non-progressive' effects of the superordination of methodological principles have been treated at length in a general way in the preceding section. It is entirely consonant with adherence to the methodological principles to make an indefinite and even unlimited number of specific alterations to a preferred theory in order to maintain that theory in the face of anomalous empirical findings. Indeed, Hull (1937) considered this unlimited alterability of methodologically rigorous theories--specifically his own--to be constitutive of their genuinely scientific character. He maintained that this feature guaranteed that such theories would progressively approach the limit of complete validity, inasmuch as negative results would be as important as positive ones in their construction and elaboration. But contrary to Hull's belief, and as we have seen, such unlimited alterability merely renders theories incapable of refutation. Furthermore, the history of neobehaviourism supports this conclusion better than it does Hull's faith. Throughout the neobehaviourist period the theories of, say, Guthrie, Hull, and Tolman--as well as the derivative ones of, say, Estes, Miller, and Krechevsky--were subject to constant experimentally based criticism, each being criticized by the proponents of the others. Such criticisms had their effect: they stimulated accommodation in each theory in order to meet the objections, and such continual accommodation was widely regarded as demonstrating the corrigible, hence progressive, character of each. But while the competing theories were thus refined to the point where, in their areas of overlap²¹, it became difficult to tell them apart in terms of their empirical content--difficult, that is, to

draw differential predictions from them--this 'empirical convergence' was accompanied by no comparable theoretical convergence or rapprochement. At the level of theory, the Guthrians, Hullians, Tolmanians, etc., remained as far apart as ever. Their shared methodological commitment helped to keep them theoretically disunified, by apparently rendering all their disparate positions empirically defensible, rather than to bring them any closer to theoretical unification.

The effects of the subordination of substantive principles, insofar as they can be separated from the effects of the superordination of the methodological ones, were more subtle. The brunt of much of the preceding section was that some kind of logically and methodologically indefensible realist commitment (or ontological reference, or substantive insight) is not merely desirable or appropriate as a basis for pursuing theoretical development, but is absolutely indispensable to it. Such commitment provides the only possible systematic basis on which many of the crucial theoretically relevant decisions--on how to interpret data, on what variables to implicate and hence control for in making operational definitions, etc.--can be made. It follows therefore, that the neobehaviourist theorists must in fact have incorporated some such indefensible references or unjustifiable substantive principles into their positions, since otherwise they could not have effected any theoretical development even of their own theories. And indeed, such indefensible references can be found throughout neobehaviourist theories, if one looks hard enough for them. And there is the rub: such references are explicitly denied--are methodologically banned--in neobehaviourist theories; being denied, they make their appearance only covertly; being covert, they are idiosyncratic to the particular theory in which they appear (or, one might say, in which they are hidden); being both

idiosyncratic and covert, they are out of the 'public domain', unavailable to other theorists either to adopt or to criticize.

What was covert about the substantive principles in neobehaviourist theories was not their presence but their methodological indefensibility, that is, their character of having content which was not in fact specifiable in terms of, or justifiable by, the methodological rules which purportedly governed the introduction and application of theoretical terms; and furthermore, their character of being usable within the various theories precisely by virtue of their unjustifiability. Almost the entire set of theoretical terms, postulates, and variables of neobehaviourist theories had covert substantive implications in this way. Their incorporation is responsible for much of what unique content remained in the diverse neobehaviourist theories, after theoretical accommodation had gone as far as it could go; it was largely the disguised independent content implicated in the use of such theoretical terms that was manipulated and elaborated in the different theories. As Scriven observes:

I remember the glee with which I discovered that nobody actually produces operational definitions, even when they say they do. Hull's work is replete with examples of allegedly operational definitions. Within three lines of many of these he will insert an ontological addendum but still insist that the defined term has no meaning except as an intervening variable (Scriven, 1964, p. 180).

The two most extensive and best documented classes of theoretical terms with such covert and unjustifiable substantive implications comprise also the two most widely used classes of theoretical terms in neobehaviourist theories: intervening variables in the theories of Guthrie, Hull, Skinner, Tolman, and their students, and hypothetical constructs in the theories of Hull and his students. Both of these classes of variables are supposed to be completely definable,

in different ways, within a theory, and their use is supposed to be restricted to what follows from their explicit definition²². In fact, they are practically never thus definable, and their use in a theory depends essentially upon the ambiguity of reference which they display, upon informal extrapolation from their formal definitions, and upon the implicit ascription to them of autonomous status to a sufficient degree that their theoretically relevant properties can later be discovered in the subsequent elaboration of the theory. This much can be said with full confidence, even without making a detailed application of the analysis contained in the preceding section to the composition of hypothetical constructs (as postulates in a hypothetico-deductive theory) and of intervening variables (as a class of operational definitions). Rather, all of this has been demonstrated in numbing detail, through consideration of these constructs independent of any general analysis such as has been offered here, by Koch (1954, assessing Hull), by MacCorquodale and Meehl (1954, assessing Tolman), and to a somewhat lesser extent by Mueller and Shoenfeld (1954, assessing Guthrie) in their contributions to Modern Learning Theory.

Furthermore, the indefensible status of these constructs was more or less attested to, again in different ways, by the final reflective statements of these theorists themselves. In his posthumous final book, A Behavior System, Hull (1952) retracted all the claims to generality of his 1943 attempt at a systematization of the laws of behaviour (cf. the internal quotation on p. 21), and while continuing to hope that a comprehensive and rigorous theory of behaviour might someday prove possible, judged that even his latest attempt "will serve mainly to call attention to the problem (Hull, 1952, p. 354; quoted in Koch, 1954, p. 168)." Tolman, more forthrightly, came eventually to repudiate the

hope of ever making a complete definition of any intervening variables and declared that, if they were not to be abandoned altogether, then at best all they could be considered was "an aid to thinking (Tolman, 1959, p. 148)." Tolman's statement is not only a remarkable conclusion from the man who introduced intervening variables into psychology and made them the basis of his 'operational behaviorism'; it also comprises a very close parallel to the almost simultaneous final judgment of Bridgman, mentioned in Chapter 1, on the value of operational analysis in general. Guthrie, most forthright of all, repudiated the very possibility of ever making a methodologically secure anchorage of theoretical terms as the basis for their subsequent use, as follows:

The fact that it had taken Russell and Whitehead some 400 pages to establish the conclusion that one plus one equals two, and that every intervening step could be challenged and would require more proof, and that the steps of these added proofs would require still more, has made me impatient with the notion that there can be any completely rigorous deduction, or ultimate validity in an argument. This scepticism colors my notions of the nature of scientific facts and scientific theory (Guthrie, 1959, p. 161).

It may be noted however that this insight did not prevent Guthrie, who was a logician before he became a psychologist, from attempting to use the intervening variable approach as a means of solidly anchoring the terms of his own theory. His statement thus represents a gradual realization, acquired over a period of forty years, that the limitations of pure logic as exemplified in the Principia Mathematica apply also to the application of logical principles in the construction of psychological theories. (Skinner, who also made some use of intervening variables and later repudiated them, is, as in many other respects, a special case, and will be considered separately in Chapter 6.)

Thus, the purpose of bringing the logical indefensibility of these theoretical terms into the discussion is not to demonstrate it in

detail, that task having already been accomplished to almost everyone's satisfaction, but to relate it to the general analysis offered here and to show its implications for the constitution of neobehaviourist theories. The way in which this subordinate and hence covert status of the substantive principles thus embedded and functioning within intervening variables and hypothetical constructs helped maintain theoretical fragmentation throughout the discipline was, as indicated, through keeping the principles from the public domain, or rather, through rendering them subtly and ambiguously different from the explicitly stated principles which were in the public domain. Each theorist tended to construe his own theoretical principles in terms of what he meant by them, what function he intended them to serve, while maintaining and no doubt sincerely believing that their operational or postulational specification provided a firm warrant for the use which he was making of them; conversely, he tended to construe the theoretical principles advanced by a competing theorist in as firm and rigid a manner as he found possible, investing such competing principles with a bare minimum of excess or unjustified meaning. Each theorist or school of theorists, therefore, tended to expect that the results of 'crucial experiments' adduced by them would prove quite inexplicable by a (rigorously construed set of) competing principles. But conversely, they continually found that the results of 'crucial experiments' advanced by their competitors could readily be accounted for on the basis of their own principles; or at least that such findings were compatible with their principles and could be accounted for by them after the inclusion into their theory of a few supplementary principles perfectly consistent with the spirit of those already present. Experimentally based arguments brought against theories by the proponents of competing

theories thus tended to pass one another by without making much contact. Each group of theorists tended to see the mutual impenetrability of each other's theories as evidence of the basic worth of their own approach and of the deviousness of that of their opponents. This pattern of off-centre criticisms and rebuttals was especially characteristic of the relationship between the most directly competing groups of theorists--who were also those having the firmest commitment to the construction of elaborate and methodologically rigorous theories--those centred around Hull and around Tolman. The long and drawn out controversy between these two groups on the subject of transposition behaviour, for instance, was conducted very largely in terms of 'crucial experiments' that were supposed to settle the issue for once and for all--but of course never did.

Hence, even the minimal 'realist commitment'--if such it can be called, since it constantly shifted and as a result did little to limit ad hoc modifiability--of neobehaviourist theorists to the principles of their own theories helped to maintain the theoretical fragmentation of the discipline, simply because it was never made clear just what the commitment was to. The strictly unwarrantable substantive principles which they adopted were inadvertently kept secret, were disguised (for themselves, it should be emphasized, as well as for their opponents) as purely explicit and hence methodologically justifiable constructions, and consequently were unavailable for systematic comparison with alternative principles. The way out of this dilemma of uncomparability, it should be clear by now, does not lie in enforcing ever more rigid rules on the composition of theoretical constructs; it was the inapplicability and ambiguity of such rules that gave rise to the dilemma in the first place. Rather, it lies in the determined effort

to specify what it is of substantive import that is being claimed by a theory, whether through rigid entailment or not; if the content of a theory cannot all be rigidly derived from postulates, it can at least be made public, as far as possible at any given time, by its proponents.

Summary of the Character of Behaviourism's Positivism.

The legitimate and valuable role that a positivist orientation can play in the conduct of scientific inquiry has been sketched out, both in the present chapter and in Chapter 3. In behaviourism, however, positivism did not perform this role, but performed one that was almost diametrically opposed, resulting in the continuing fragmentation of the discipline. Now that the characterization and analysis of positivism have been completed, it may be well briefly to review the factors involved in the systematic differences between positivism in behaviourism and scientific positivism in general. These factors were all operative in the relatively primitive data-base positivism of classical behaviourism, and continued to be operative without significant modification apart from their further development and elaboration, in neobehaviourism.

As indicated in Chapter 4, we can distinguish two versions of positivism present in the founding of behaviourism. The first is exemplified by Watson's statement that

It seems reasonably clear that some kind of compromise must be effected: either psychology must change its viewpoint so as to take in facts of behavior, whether or not they have bearings upon the problems of 'consciousness'; or else behavior must stand alone as a wholly separate and independent science (Watson, 1913a, p. 159).

The second is exemplified by his statement, only a few pages further on, that

The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation (ibid., p. 163).

The first statement constitutes a declaration that in the area in which Watson was working, that of studies of animal behaviour, the dominant conceptual framework had become so cumbersome and unworkable that it needed to be sloughed off, in order to allow undirected and hence unimpeded concentration on experimental studies. This first version of positivism was thus a purely internal development--internal to comparative psychology that is--and was, on the analysis presented here, a perfectly appropriate response to the unresolvable conceptual dilemma in the field. The second version, however, as represented by Watson's second statement, was very different. It constituted a kind of intellectual imperialism, an unrequested extension of the indigenous positivism of comparative psychology to the discipline as a whole. The circumstances which led to this step, and those which led to its gradual acceptance throughout psychology, were both unique. Both were detailed in Chapter 4 but may briefly be reviewed here from a slightly different perspective.

The circumstances which led to the extension of positivism throughout the discipline were related to that same conceptual framework of functionalist comparative psychology against which Watson was, from a different direction, rebelling. That is, the sensationalism of functionalist comparative psychology became, with the local excision of consciousness, environmentalism. Environmentalism, in turn, is a general position; it cannot be applied to animals and not to man, unless man is credited with an immaterial mind or soul, completely different in kind from whatever it is that animates animals. Thus, if environmentalism is established by the rejection of consciousness as an object of study, it is inevitable that the position be extended to include man. The alternative would be an uncompromising dualism established for man

alone, a dualism which would therefore seem to have inevitable theological implications²³. Neither the substantive nor the research-based methodological arguments in favour of Watson's position of environmentalism and the repudiation of consciousness could carry enough weight to gain general acceptance in the field of human psychology of the time however, despite what would seem from the standpoint of comparative psychology to be their universal applicability. Thus the more general methodological argument derived from the alleged practice of physics, concerning the general requirements of objectivity, was of prime relevance in establishing Watson's position. This argument was one which could legitimately be applied universally, without requiring experimental validation at each step (such is the apparent advantage of methodological arguments); it was a generally positivist argument in that it applied to all unobservables equally. Thus, the unique circumstance leading to the move to extend the positivist reaction throughout psychology was that of the unique relevance of a general methodological argument to the establishment of a position that had both methodological and substantive components.

The circumstances which led to the gradual and general acceptance of the extension of positivism throughout the discipline--and which eventuated in the complete separation of environmentalism (and later even the wholehearted rejection of consciousness) from the methodological position--were likewise unique. They comprised the growing positivist orientation of the whole scientific culture, a positivist reaction that was of unprecedented extent in the history of science. This reaction was occasioned by the overturning of Newtonian mechanics, with the immediate effects described in this chapter and in Chapter 3, and was maintained by the intuitively incomprehensible

findings of quantum mechanics and, to an only slightly lesser degree, relativity theory. While formally positivist philosophy--except for the writings of Mach, which were widely misunderstood to be idealistic--did not have a wide currency in the United States during behaviourism's early years²⁴, that is, until some time after Bridgman introduced operationism in 1927, there was a widespread appreciation of what seemed to be the limits of scientific explanation, as consisting in closely determined empirical generalizations of observed data with relatively little systematic import²⁵. These circumstances accounted in large part both for the growing popularity of a positivist approach to psychology and for its steadily accelerating divorce from any specific systematic issues.

The fact that behaviourism's positivism was thus an importation from outside psychology, and that it was likewise maintained largely on the basis of external factors--that is, that it was neither introduced nor retained purely as a response to internal problems--had significant implications for both its critical and its mediating functions, as these functions were described previously in this chapter.

The critical function of positivism was not, after the beginning, directed toward specific concepts and variables that had been identified as troublesome. Even at the beginning, when concepts indicative of mind and consciousness were being criticized, the critical function was directed toward such mentalistic concepts primarily as they occurred in human psychology, rather than as they occurred, most problematically, in comparative psychology. Thereafter, the critical function was exercised as a kind of a weapon, directed at any concepts that appeared to be gaining a central role in non-behaviourist psychological theories (e.g., the concept of instinct), and eventually came,

with the development of neobehaviourism, to be applied in blanket fashion to all theoretical concepts. The critical function of positivism was thus divorced from its specific context of application to genuinely problematic concepts. In being required to be universal, the application was inevitably haphazard and unsuccessful. But although such application was unsuccessful in finally ridding psychology of all theoretical terms with empirically unspecifiable references, it forced the unwarrantable substantive principles which it failed to dislodge completely to become covert and separated from their central position in the development of psychological theory, as we saw above.

If the critical function of positivism was thus misdirected and overextended, the mediating function was, by contrast, almost entirely absent, both from classical behaviourism and from neobehaviourism. It is significant in this respect that hardly any behaviourists ever made a definite repudiation of realism, in either its common sense or its philosophical varieties, even though they adopted many of their methodological formalisms from a movement--logical positivism--which was expressly based in large part on this repudiation. While a pro forma rejection of realism specifically as a metaphysical position was sometimes made (e.g., by MacCorquodale & Meehl, 1948), the general assumption seems to have been that use of rigorously objective methods at all levels of investigation would suffice to guarantee that behaviourist theories would gradually become as true as any scientific theories could possibly be--without much detailed concern over just how true that was (such a concern being, itself, metaphysical, or at least philosophical). Thus, behaviourism hardly ever adopted or, more to the point, developed and acted upon, any conventionalist analyses of the status of theories as freely chosen 'conventions'--analyses which exemplify the mediating

function of positivism--and never acquired the freedom which such analyses can confer, freedom to engage in unrestrained flights of creative imagination. Instead, the scientific enterprise was constrained to be self-consciously pedestrian from the outset. Every theoretical development had to be such that it could be perfectly valid, and thus was expected to proceed in accordance with the rules of rigorous theory construction. As a result, rather than providing a useful place for unfettered imagination--which could then be brought down to earth by closely controlled experimentation--the development of theory was to be at all times what Hull (1937, p. 31) described as a "long and grinding labor". It was partly due to the limitations thus imposed on the character of science that behaviourist theories, while highly receptive to outside ideas that could be rendered objective, were not particularly marked by the emergence of radically new ideas of their own. Similarly, the insistence on 'objectivity', as having a firmer and more distinctly regulative character than many philosophical positivists could themselves grant it, militated against an emphasis on creativity or its product--creative ideas--as being most fundamental to the development of theories; and at the same time, this insistence promoted the entirely erroneous dependence on the notion of 'crucial experiments', as we saw above, a dependence that might well have been obviated had behaviourism's positivism been tinged with more conventionalist insights.

Chapter 6

Conclusion: Toward a General Evaluation of Behaviourism

We now come to the question of what implications the fairly specific analysis presented in this monograph, concerning the origins and systematic foundations of behaviourism, has for a general evaluation of the movement in what will eventually come to be seen as its historical context; what implications, that is, does it have for an assessment of both the positive and the negative contributions of behaviourism to the prospective development of post-behaviourist psychology? The analysis does of course have some such implications for a general evaluation of behaviourism, but drawing the implications will require ranging through the career of behaviourism somewhat more widely, and hence less deeply, than in previous chapters. Furthermore, it will have to be recognized that while there are some important respects in which the analysis can serve as the basis for such a general evaluation, there are other, no doubt equally important, respects in which it cannot. Let us make these explicit.

The analysis given here has concentrated on explicating the systematic foundations of behaviourism. Fairly definite conclusions have already been made concerning those foundations, and they can readily be extended to apply to theoretical systematizations which more or less explicitly depend on those foundations. The conclusions cannot be applied, however, to the evaluation of specific pieces of behaviourist research, for these gain their chief significance only in their systematic context. The negative judgment which has been made on much of that context certainly reflects on the research conducted within it, but does not apply directly to any of the individual pieces of research themselves,

insofar as they can be considered separate from their context. Neither can any of the conclusions already arrived at be applied, except very conjecturally, to those contemporary positions in psychology which are often designated as 'behaviourist' but which share few, if any, of the systematic characteristics of either classical behaviourism or neobehaviourism. Finally, in the attempt which will be made to describe the chief positive contribution which behaviourism has made to the ongoing development of psychology, it will become necessary to go somewhat beyond the analysis presented so far and to consider briefly some aspects of behaviourist research which were independent of, and almost unaffected by, any systematic considerations.

I. Systems and Systematic Methodology in Behaviourism.

The fundamental systematic contribution of behaviourism lies in its practical demonstration of the untenability of the methodological principles on which it was founded. This may seem to be a harsh and negative judgment, but it should be emphasized that the contribution was a major one. As has been stressed many times throughout this monograph, behaviourism represents the only--or at least by far the most detailed, uncompromising, and sophisticated--serious attempt ever made to construct a science on methodological principles alone. Constructing a science in such a way has long been a dream of philosophers and methodologically oriented scientists, but has never before been undertaken in detail. That the attempt failed utterly at two levels--that the attempt to keep ontological references at bay by means of formal techniques was unsuccessful, and that even the attempt sufficed to prevent the scientific enterprise from progressing--has or should have profound consequences for our appreciation of the complementary roles of method and substance in science.

Philosophers have often maintained that the philosophical and broadly methodological or logical presuppositions embodied in a scientific investigation should be made explicit, since otherwise they will influence the investigation in ways that escape detection and possible control. Such a claim may sometimes be invalid; when philosophical and logical presuppositions are made explicit they may exercise far greater control than when they are implicit, and if such presuppositions cannot be successfully organized into, and implemented as, a coherent formal system they may, in their explicit form, prove sufficient to hamstring scientific inquiry--as we have seen. On the other hand, it may be concluded from the examination given here that the substantive features of scientific inquiry--what it is about, and what it adduces as causal or otherwise explanatory factors--should be made as explicit and open as possible, regardless of the formal status of such features. This conclusion would be entirely unexceptionable and even uninteresting, were it not that many of the substantive features of neobehaviourist research were indeed covert and, being covert, were prevented from playing a central role in the process of inquiry.

In general, we can say that, of course, a shared commitment to both substantive and methodological principles is requisite to the cumulative development of science. But while substantive and methodological principles are each necessary, the former are of greater importance, both because they are what scientific inquiry is addressed to developing, and because they alone can provide the crucial implicit indications of when and how the inevitable deviations from the methodological principles should occur. Even in the context of reconstruction, when positivist methodological analyses are unquestionably called for, they need to be

directed by independently formulated judgments of what particular substantive principles are responsible for the science's difficulties, since otherwise the analyses will be random and haphazardly disruptive. The directive function of methodological considerations needs, therefore, to be subordinated to the particular substantive issues of individual cases in science. It is for this reason that it was intimated in Chapter 5 that there are basic limitations on the positive conclusions that could be drawn from the analysis presented there, that is, basic limitations on the regulative capabilities of any general logical or methodological analyses and of the principles which follow from them. There is no methodological substitute for good ideas, and no guaranteed methodology for acquiring them; and while methodological tools are unquestionably necessary for comparing, interrelating, and developing these ideas, the continuing progress of science is possible only if the tools, rather than the material to be worked, are ascribed the supportive function. The systematic contribution of behaviourism, therefore, consists principally in demonstrating the general applicability of these points, or rather, in demonstrating the invalidity of their contraries.

Following on the above, and in relation to the ostensible substance of the neobehaviourist attempts to systematize psychology, I think it can fairly be judged that the ambitious systematizations of Hull, Tolman, Guthrie, and their students have, apart from what was just discussed, little or no enduring significance. To the extent that they constituted the road leading to certain formulations in contemporary psychology (e.g., Miller's psychobiology, Bolles' cognitive motivational theory), and to the extent that these contemporary formulations prove of enduring merit, then the grand systems will have played a worthwhile

propaedeutic role; but such a possible and as yet undetermined historical significance provides only the most tenuous vindication for the enormous amount of effort expended on the systematizations. The basic explanatory principles advanced in these systems likewise do little to vindicate them. These principles--reinforcement, drive reduction, cognitive maps, expectancy, contiguity, etc.--were for the most part not original, but consisted in objective reformulations of explanatory principles acquired from physiologists, philosophers, other psychologists, and common sense. It cannot even be said that the failure of the grand systems had the effect of demonstrating the invalidity of such explanatory principles in the explanation of complex human behaviour, since the methodological characteristics of the systems were sufficient on their own to vitiate them--although the extreme use made of these principles in the grand systems may well help to make such principles less popular in subsequent psychological theories.

II. Contemporary Varieties of 'Behaviourist' Theory.

It will have been observed that all of the comments made about behaviourist theories, relating to the attempt to develop an explanation for their failure, have been directed to theories that have already been seen on other grounds to be inadequate--except for a few occasions relating to proposed theories which have been direct continuations of the methodologically based systematizations of an earlier era (e.g., the comments on Smith, 1969, and Skinner, 1971, in Chapter 1). This diffidence does not merely reflect scholarly humility. Rather, it expresses one of the themes that has been implicit throughout this monograph, that the progressive development of scientific theories can occur through the implementation of a wide variety of specific

methodological orientations, so long only as these are not taken sufficiently seriously as to be accorded more weight than the substantive principles which scientific inquiry adduces. This consideration severely limits any attempt at a general assessment of most contemporary varieties of behaviourism. Most contemporary varieties that is, do not rely on the systematic methodological foundations of neobehaviourism, nor on the narrowly anti-mentalistic positivism of classical behaviourism. Rather, they are constituted as behaviouristic only minimally, by the decision--based on grounds of personal preference and historical familiarity--to avoid as far as possible the use of introspective and impressionistic methods of investigation. It is only in terms of their historical context that such minimally related positions can be described specifically as 'behaviourist'; in many cases there is nothing more about them that can specifically be related to any of the systematic features of behaviourism than there was in the position of J. McK. Cattell, quoted at the beginning of Chapter 5. Any detailed evaluation of the models and systematic theories associated with such minimal contemporary varieties of behaviourism must be based, therefore, on consideration of the specific content of the theories and of the experiments which provide evidence in their support. To the extent that the content of contemporary 'behaviourist' theories has not been subordinated to methodological considerations, and complementarily, to the extent that the methodological character of such theories has not been adduced as justification for them more or less independent of their content, they cannot fairly be evaluated on the basis of such methodological considerations.

And yet, that much said, one can scarcely avoid acquiring some impressions--and it must be emphasized that they are only impressions--

about the way that even the slight methodological constraints of much contemporary behaviourism have subtly influenced it. Consider the approach taken by such sophisticated contemporary behaviourists as Hebb, for whom 'CNS' has meant conceptual nervous system for 25 years, and who has developed elaborate models of the ways in which cognition, self-consciousness, and even moral sentiments may be represented in such an idealized structure; or Eysenck, who has developed wide-ranging theories of personality dynamics based on the operation and interplay of a small number of inborn personality traits; or Broadbent, whose research activity is directed almost entirely to discovering the basic structures of cognitive functioning; or Berlyne, who has made pioneering studies of humour and of the aesthetics of artistic experience. The work of these psychologists constitutes much of the very best of contemporary psychology. All of these psychologists account themselves behaviourists, or 'methodological behaviourists', and in the sense given, that of avoiding subjective and introspective methods and using observations of behaviour as such as their sole source of data, they undoubtedly are. And yet it is difficult to escape what is, again, an impression, that 'behaviour' functions for these theorists chiefly as a metaphor in terms of which human activity as a whole can be elliptically described, and that they employ it because they find observations expressed solely in terms of this metaphor to be personally congenial--and that they find them congenial because, again, of the historical familiarity of the metaphor and because of the lingering belief that, despite everything, observations of behaviour, even though it is often highly complex verbal behaviour, are, as long as they are nonetheless purely behavioural, possessed of some unique scientific validity. It is hard to account for why it is that theorists concerned with concept

formation, personality dynamics, and aesthetics, eschew any direct involvement with the personal--subjective--experience of their human subjects--who, unlike animals, can tell them about it--except on this basis of the unjustifiable assumption that behaviour data are the only reliable and scientifically valid ones, and conversely, that introspective and impressionistic data are incorrigibly untrustworthy. To the indeterminate extent that these psychologists and other contemporary behaviourists continue to rely solely on behaviour data on the basis of such considerations as these, their methodological behaviourism represents no more than the lingering traces of the behaviourist anti-mentalist prejudice. This is not, however, to condemn these psychologists in any way; we all need a perspective, a point of view, from which to consider human activity, and their perspective, resting on the behavioural metaphor, has become sufficiently broad that it shuts no more from their field of view than many another perspective might do. But it is to insist that if the suggestion made here as to the basis for their behaviourism is at all valid, then the reliance of these psychologists on the behavioural metaphor cannot be accounted more than a purely personal preference, without any particular scientific justification--a preference that may be explained, but not scientifically justified, by psychology's history. Thus, while the influence of these psychologists is unquestionably great, it may be too much to expect--and hardly something to hope for--that their personal choice of investigative methodology will exercise a continuing directive influence on those who follow in their theoretical footsteps.

III. Unsystematic Positive Contributions of Behaviourism.

Apart from any strictly theoretical considerations, the practical applications of behaviourist psychology in the field of

behaviour modification may be cited as among its chief contributions to psychology in general. Various techniques of behaviour modification have had great and unquestioned application in therapeutic, educational, and other contexts; they are sufficiently widespread and well known as to render a review unnecessary. However, without questioning either the significance of behaviour modification techniques or the close association that they have had with behaviourist psychology--but, on the contrary, to explicate that association--one could legitimately question the extent to which the introduction and application of behaviour modification techniques constitutes an outgrowth of behaviourism. Behaviour modification, in its principal early form of behaviour therapy, started to become influential with its practical development by psychiatrists disillusioned with psychoanalytic techniques, primarily Salter (1949) and Wolpe (1952), and was initially based on a muscular relaxation technique developed by the physiologist Jacobson (1939). Behaviourist psychologists quickly enough became interested in the possibilities of behaviour therapy and refined the therapeutic techniques, but were not responsible for introducing it as a method of treatment.

Prominent behaviour therapists such as Wolpe and Eysenck have typically maintained that behaviour therapy constitutes an application of Hullian and Pavlovian theory. However, since the critical review of the ostensible theoretical basis of behaviour therapy by Breger and McGaugh (1965), and the resulting controversy, it has become very dubious as to whether behaviour therapy owes its formulation to behaviourist theory any more than it owed its initial dissemination to behaviourist practice. Indeed, the debate on the theoretical underpinnings of behaviour therapy has faded recently, since it became generally recognized that the techniques are far more viable than any

of the theory on which they are purportedly based. London (1972) celebrates this developing autonomy of the techniques as signalling 'The end of ideology in behavior modification', and points out that dependence on outdated behaviourist theories is something that behaviour therapists may be better off without.

Finally, if both the theoretical basis and the practical introduction of behaviour therapy cannot be unhesitatingly ascribed to behaviourist psychology, then neither can its first demonstration. The demonstration by Watson and Rayner (1920) of conditioned and deconditioned fear responses in 'Little Albert' is often cited as the first exemplary case of behaviour therapy (e.g., by Wolpe, 1969). However, Freedberg (1973) has shown that a well developed tradition of behaviour therapy existed in the United States from about 1890 onwards, with Morton Prince and Boris Sidis as its chief innovators. The techniques used came, by 1909, to be held to derive broadly from Pavlovian theory, although the theoretical derivation was no more straightforward then than it is now. The techniques were comparable both in procedure and in efficacy with some of the early modern ones, although they were not accompanied by the modern de-emphasis of conscious processes in their implementation--a de-emphasis that, as Breger and McGaugh (1965) have shown, is in any case strictly pro forma.

None of this is to deny, however, the immense significance of the contribution of behaviourist psychologists in developing, extending, applying, further disseminating, and validating the various techniques used in different forms of behaviour therapy, as well as in other, derivative, types of behaviour modification. Indeed, the features of behaviourism which enabled it to serve as the basis for developing and extending these techniques constitute, I suggest, the

principal contribution that behaviourism has made and can continue to make to the future of psychology. These features which enabled behaviourism to perform this role are not, however, amongst the ones which are typically most highly emphasized in accounts of behaviourist psychology. They are features which emerged from the animal laboratories, from work which was initially the most remote from any areas of human concern, and they constituted some of the specific aspects of that work which were the most remote also from the system-building considerations which were the primary focus of interest of most neobehaviourists. They can best be described in a slightly roundabout way, through a brief discussion of the psychology of Skinner, in which, out of all the highly developed versions of neobehaviourism, they figure most prominently.

IV. The Principle Unsystematic Contribution of Behaviourism as Exemplified by Certain Features of Skinner's Psychology.

Skinner has never professed adherence to any of the trappings of the hypothetico-deductive method, and until very recently abjured systematic theorizing of any sort. He clearly figures as a neobehaviourist nevertheless, because of his uncompromisingly methodological orientation to psychology. As mentioned in Chapter 2, he was possibly the earliest exponent of operational definitions in psychology, although he did not publish on the subject until some time later¹. Even more than any of the other neobehaviourists, Skinner insisted that the proper employment of rigorous methodology, both at the experimental and at the systematic level, was necessary and sufficient to the construction of a scientific psychology. He merely found no advantage in going beyond simple empirical generalizations in his reporting of results. His one bow to the fashions of neobehaviourism, the intervening variable concept

of 'reflex reserve' in his 1938 book, was later described by him as

...an abortive, though operational, concept which was retracted a year or so after publication...It lived up to my opinion of theories in general by proving utterly worthless in suggesting further experiments (Skinner, 1959, p. 369).

Consonant with his rejection of formal theories, the covert substantive principles in Skinner's system do not occur chiefly as specific postulates or theoretical concepts with disguised ontological references. Instead, they function at a higher level, as basic meta-systematic orienting assumptions. These assumptions are fairly prominent in Skinner's system, and indeed, account for all of its general systematic character.

The meta-systematic orienting assumptions in Skinner's psychology amount to the assumptions of environmental and speciational generality. They are assumptions which were to some degree characteristic of all neobehaviourist theorizing, and as a result the discussion of them here can serve as a supplement to the general discussion of covert substantive principles in neobehaviourist theories as presented on pp. 300ff., but they were present in none other so forcefully as in Skinner's system. The assumption of environmental generality, to put it excessively crudely, asserts that the Skinner box is representative of all environments. The assumption of speciational generality, equally crudely, asserts that the pigeon is representative of all species of organisms. The two assumptions together provide a warrant for extrapolating from the behaviour of pigeons in Skinner boxes to the behaviour of all animals in all environments, and specifically to the behaviour of humans in complex social situations.

This caricature of Skinner's assumptions serves to convey their import, but, that accomplished, they should in all fairness be

stated less crudely. In Skinner's particular version of them (which, it should be made clear, is a reconstruction from Skinner's practice, not a representation of any of his systematic statements), they are actually second order, rather than first order, assumptions, serving to indicate the procedures required for extending the range of a descriptive schema. The assumption of environmental generality is more properly that the modifications in descriptive terminology which are necessary in order to extend a description of behaviour displayed in a Skinner box to cover that displayed in another, dissimilar, experimental environment, such as a runway or a slide, in which the stimulus features to which the animal responds are different, suffice to ensure the adequacy of the description (or rather, the descriptive schema of which the description is an instantiation) when applied to behaviour displayed in any environment. The assumption of speciological generality asserts that the modifications in descriptive terminology which are necessary in order to extend a description of behaviour displayed by one species to cover that displayed by another, unrelated, species, suffice to ensure the adequacy of the description (or descriptive schema) when applied to any species. The upshot of the two assumptions is thus that a descriptive schema which proves adequate to characterize the behaviour of rats and pigeons in two different kinds of Skinner boxes is adequate to characterize the behaviour of all organisms in all environments.

As indicated, these orienting assumptions are the basis for the systematic status--or systematic pretensions--of Skinner's psychology. Once identified, they can clearly be seen to be invalid. With regard to the assumption of speciological generality, it is plain that there are major differences in the pattern and structure of behaviour

displayed by different species even in highly controlled and comparable experimental environments. These differences are sufficiently great that they render descriptive rubrics applied to the interchange between organism and environment irrespective of species--such as 'reinforcement', applied, again, purely as a descriptive term--only trivially applicable. That is, the behaviour of organisms in controlled operant environments varies so greatly as a function of species differences--for instance it is relatively easy to condition a rat to make a standard operant response (a bar press) for shock avoidance, but almost impossible to condition a pigeon to make a standard operant response (a key peck) for the same reinforcer--that the description of such behaviour by means of general rubrics such as 'reinforcement' and 'response shaping' forces the ignoring of much of the observable and systematic variability of the behaviour. Cogent criticism of various forms of the assumption of speciational generality was advanced by Beach (1950, 1955), but in a context that made the applicability of his criticisms to purely descriptive formulations only tenuous; furthermore, his criticisms did not focus on the behaviour of different species in similar, controlled environments. By contrast, Seligman (1970) has surveyed a wide range of experimental results that make the criticism incontrovertibly applicable to the description of behaviour emitted in Skinner boxes.

The assumption of environmental generality is, if anything, even more clearly fallacious. That is, it may be possible to restrict the experimental environment sufficiently that some limited, but specific and useful, descriptive generalizations can apply to a broad range of species within such environments. With the relaxation of rigid restrictions on the composition of the environment, however, the variability of behaviour even within a single species becomes so great

that the only descriptive generalizations that can be made applicable throughout the range of environmental conditions are those so broad and diffuse as to be practically meaningless.

For instance, Breland and Breland (1961, 1966) have presented a wide variety of examples of well established conditioned behaviour chains which became severely disrupted when the experimental animals were removed from the original conditioning apparatus and placed in a complex situation more closely resembling the animals' natural environment. In the more complex situations the animals consistently interrupted their conditioned response chains with behaviour segments which the Brelands considered 'instinctive', which were never reinforced by them, which prevented the animals from receiving reinforcement, and which were very highly resistant to extinction. In many cases, furthermore, the animals proved very recalcitrant to 'reconditioning' in the more complex situation, but impossible to condition initially at all except within the control apparatus. The Brelands concluded that the language of operant levels, reinforcement contingencies, etc., was utterly inadequate to the description of behaviour in complex situations as observed by them; that is, application of this descriptive terminology simply did not permit them to describe what was going on.

Furthermore, the invalidity of the assumption of environmental generality can be demonstrated, again, even within the confines of a Skinner box, by systematically varying the parameters of the experimental situation. Even within a controlled operant environment, the behaviour of a given organism under a given reinforcement contingency with a given (operationally defined) motivational state will vary systematically as a function of the specific response chosen for examination, the specific discriminative stimuli serving as cues for the emitting of the response, and the specific reinforcer contingent upon that response. To give a

simple example, it is fairly easy to condition a pigeon to key peck with food as a reinforcer, but very difficult to condition it to do so with shock avoidance as a reinforcer. To cite a more elaborate example, the relationship between choice of discriminative stimuli and choice of response in dogs, with reinforcer held constant, has been elegantly demonstrated. The experimental situation is basically one in which dogs are trained to make a stimulus discrimination; the discrimination serves as a cue to indicate which of two responses the dog will be reinforced for performing. The experiment is a 2 x 2 factorial one, with two sets of discriminative stimuli (location of tone as coming from upper or lower speaker vs. pitch of tone as higher or lower) and two responses differentiations cued by the discriminative stimuli (choosing the correct alley of a T maze vs. running or not running down a runway²); a different group of dogs is assigned to each of the four combinations of stimulus discriminations and paired response choices. What is found is that dogs will learn to discriminate differences in tonal pitch much more readily than differences in tonal source when the discrimination cues a go-stay response choice. Conversely, they will learn to discriminate differences in tonal source much more readily than differences in tonal pitch when the discrimination cues a left-right response choice³ (Lawicka, 1964; cited in Seligman, 1970). Again, the descriptive terminology of reinforcement contingencies, operant levels, response shaping, etc., is entirely inadequate for describing the behavioural variability observed in such experiments. Such terminology is thus insufficient for providing descriptive generalizations about behaviour manifested in different environments, even when the environments are varied only in extremely specific and highly controlled ways⁴.

Evidence such as this exposes the systematic pretensions of

Skinner's writings, whereby he extends his descriptive account of the behaviour observed in his own animal studies to cover complex human behaviour (e.g., Skinner, 1948, 1953, 1957, 1971) as thoroughly unjustifiable⁵. The meta-systematic assumptions on which Skinner's systematic extrapolations are based have been treated at some slight length, so as to contrast them vividly with the other type of his covert principles, broadly methodological ones in this case, relating to the perceptual specifiability of stimulus and response. Again, these features are, like the assumptions just considered, characteristic of other neobehaviourist approaches to psychology than Skinner's, but not to such an extent. To treat these covert methodological principles, it will be necessary briefly to reconstruct the way in which Skinner's approach to psychology can be considered to serve as the basis for naturalistic and purely descriptive formulations.

Despite the impression one sometimes receives, Skinner is not-- or at least was not (his latest, 1971, book is ambiguous, even on close reading)--an environmentalist, in the sense of assuming that environmental pressures cause all behaviour. His position in this regard is not simply a positivist disavowal of 'causes'⁶, but accounts also for his total disinterest in physiology. Whether or not behaviour is caused by the environment, it takes place in an environment. Any recognizable piece of behaviour can be observed in its environmental setting. The behaviour may occur randomly with respect to all discernible features of the environment--as, for instance, some tics may occur. More typically, a piece of behaviour may occur more frequently under some environmental conditions than under others. The environmental conditions include anything that can be described in the environment, so that the absence of females in mating season, for instance, comprises part of the

environmental conditions under which certain birds are more likely to exhibit vacuum courtship activity. Description of the environmental conditions under which a piece of behaviour is most likely to occur comprises the most important part of a description of that behaviour in relation to the environment. Whether the environment forces, compels, elicits, potentiates, or provides an opportunity or a cue for, that behaviour, is irrelevant to the description of the behaviour as occurring in the environment.

When a piece of behaviour occurs, it usually has what we describe as an effect upon the environment. That is, some of the environmental conditions change either concomitant with or subsequent to the behaviour. Opening a door is an example of the first sort; the change in the environment is inseparable from the behaviour. Typing is an example of the second sort; the behaviour of hitting a key is generally followed by the action of a type die hitting the paper. In almost all cases of the second sort (as well as in some of the first, such as turning on a light), the change in environmental conditions is perceptibly separate from the behaviour of which it is considered an effect. For the change in environmental conditions in such cases to be identifiable as an effect of the behaviour (since the environment is constantly changing), it is therefore necessary that it be observed to be subsequent to the behaviour (or concomitant with it) on more than one occasion⁷.

Certain changes in environmental conditions--that is, certain newly observable environmental conditions--which we thus describe as effects of behaviour, may be among the discernible conditions under which the behaviour can be identified as most likely to occur. That is, the conditions under which the behaviour is most likely to occur

may include some conditions only insofar as they can be identified as effects of a previous occurrence of the same behaviour (in a sense of 'same' which is yet to be explained). Such conditions, if present other than as effects, are not ones under which the behaviour is most likely to occur. Description of these conditions as effects of a previous occurrence of the same piece of behaviour thus comprises an important part of the description of that behaviour in relation to the environment in which it occurs. Many pieces of behaviour may in this way be described as being most likely to occur under conditions which are effects of a previous occurrence of that behaviour. It is possible to attempt to discover, through careful observation, whether the effects of behaviour always constitute some of the conditions under which the behaviour is subsequently most likely (or, mutatis mutandis, least likely) to occur. To the extent that such relationships--between the effects of behaviour and the subsequent occurrence of that behaviour--can be discerned, they can be elaborated into systematic statements of the relationship between behaviour, behavioural effects, and environment, without in any case either addressing or begging the question of the causes of that behaviour.

Now, all of this is a very roundabout way of saying that the consequences of behaviour may reinforce that behaviour and that the composition of the behavioural repertoire may to some indeterminate extent be described as a function of previous reinforcement contingencies. The roundabout way of expression serves, however, to indicate the descriptive character of (at least much of) Skinner's psychology. Tracing and manipulating the observable regularities is the sole focus of concern. It is important to establish this point, because it is only insofar as Skinner's psychology--and by extension other varieties of

behaviourism and neobehaviourism--can be regarded as purely descriptive, or as having purely descriptive components in the sense of this reconstruction, that the following comments can apply.

We have seen that the descriptive schema which Skinner developed in line with the foregoing reconstruction cannot in fact be applied with full generality, even before we consider human behaviour. But that is not the point. The point is that for the description to be applied at all, it is necessary to be able to identify the referents of the terms of it, the 'pieces of behaviour' and the 'environmental conditions'. This identification cannot be done--or at least it has not been done--in any consistent and formal way. Rather it involves a training of perception, the acquisition of the ability to see stimuli and responses, and the subtle effects of one on the other, in the flux of environmental conditions and organismic movements.

This training of perception is a subtle process, requiring considerable exposure and practice in the observation of experimental situations. It has been the subject of almost no systematic investigations (at least within psychology), but constitutes the 'apprenticeship' aspect of the training of experimental psychologists. That is, the final product of a simple conditioning experiment, such as a bar press by a rat, is usually identifiable by physical criteria such as the depression of the bar to a criterion depth. But the shaping of the bar pressing response requires close and trained observation of the behaviour of the rat in the Skinner box, to serve as the basis for recognizing and reinforcing the rat's successive approximations to the desired response. Even closer and more sophisticated observation is necessary in order to shape the complex trick behaviours of demonstration animals, such as the playing of ping-pong by chickens--or the guiding of missiles

by pigeons (Skinner, 1960).

The work of Skinner and his students has been characterized, out of all the varieties of neobehaviourism, by the most highly developed, sophisticated, and detailed forms of such trained and resultingly skilled perception, because of the emphasis of these psychologists on precise descriptive formulations and on control of the fine grain of the behaviour of individual animals, and conversely, because of their distaste for abstract theoretical structures⁸. To a somewhat lesser extent⁹, however, this acquisition of perceptual skills has been characteristic of the training of experimental psychologists, especially animal psychologists, of every stripe. The acquired ability to see stimuli and responses, independent of the physical specification of either--the skill involved in being able to recognize what groups of motions of an experimental animal have the necessary integrity or 'grouped-togetherness' to serve as the unit of description and control--has been the almost entirely unacknowledged foundation on which all behaviourist practice has been built. The unique contribution of Skinner and his students has been to elaborate and ramify this foundation to a greater extent than any other psychologists have done--and it is a major contribution, because the foundation of perceptual skills remains intact when all of the systematic edifices built upon it, including Skinner's own, have crumbled.

This emphasis on the acquisition of perceptual skills helps to resolve one of the many longstanding anomalies in the theory and practice of behaviourist psychology, that concerning the identification of stimuli and responses. The complaint has often been brought that although behaviourists have insisted on a purely objective observation base, they have never been able to define it, and their usage of the

terms 'stimulus' and 'response'--fundamental to that observation base--has been ambiguous and inconsistent. Much of the problem has, indeed, stemmed from the various attempts to make explicit definitions of what was to count as stimulus and response, with the problems being especially great in the case of the latter. Koch makes some references to these problems in the quotations drawn in the first section of Chapter 5. In practice, however, there was little or no problem in the use of these terms. Stimuli and responses are what one has learned to see as stimuli and responses. They are not initially, but they become, directly observable.

Training in ways of seeing--acquiring skills in the recognition of the functional significance and structural integrity that constitute physical movements as responses--learning to recognize the meaning of behaviour, as established by these features of it, as amongst the givens in what one is observing: these processes constitute training in what can only be called phenomenological analysis. The analysis is phenomenological in that it involves a subordination of one's preconceptions to the situation that is being observed, in order to discover the perceptual givens in the situation. The practice--as opposed to the theory--of behaviourism is thus in this sense based on phenomenological analysis, and has been, furthermore, from the beginning.

It may, of course, seem outlandish to suggest that behaviourism is based on phenomenological analysis. The apparent outlandishness, however, is due to little more than the fact that the two are not customarily considered to be related, and more specifically, that phenomenology is often considered to have close affinities with subjectivism and--most curiously--introspection. Nothing can be done about the unfamiliarity of the mentioning of phenomenology and behaviourism

in the same breath, but the oft-assumed relationship of phenomenology with subjectivism and introspection can easily be disposed of.

Phenomenology is a way of looking; it does not specify the direction of looking. Introspection can be, but need not be (and usually is not) phenomenological, just as 'extraspection' or looking at the world can be, but need not be (and usually is not) phenomenological. Phenomenology involves merely the setting aside of our abstract and intellectual interpretive schemas in order to discover what is given in perception and how it is organized, apart from our knowledge of it. The 'givens' can be either those that are given as internal or those that are given as external to the self.

Neither does phenomenology necessarily involve any blurring of the boundaries between the self and the world. To be sure, the expressive power of a painting, say, may be such that we 'read into it' qualities of warmth, spirituality, foreboding, or whatever; and in a phenomenological description of the painting these qualities will typically be referred to the painting even though we know that they are 'really' constituted by our reaction to the painting. But this is not a blurring of the boundary between self and world; on the contrary, the dasein of the object, or its quality of being-over-there separate from the self, turns out to be one of the most general givens in any phenomenological account of objects. The fact that a phenomenological account may ascribe properties to objects that they do not 'really' have, as in the example given, may seem to make such accounts of scant utility in providing a basis for dealing with such objects. But again on the contrary, it is precisely this feature of phenomenological accounts that makes them relevant; the status of stimuli and responses as being given in perception is what makes it possible to deal with them, despite

the impracticality or even impossibility of making an unambiguous, purely physical specification of them.

Still, it may be objected that the process of learning to see stimuli and responses is in no way a phenomenological process at all, is if anything the very opposite of a phenomenological process, since it is artificial--as evidenced by the fact that it requires directed training--and hence amounts to imposing a conceptual schema on the objects of inspection. There are two replies to this objection. The first is that it is not at all certain how widely such imposition occurs; among the Skinnerians in particular, close observation has typically preceded any description. The second is that it doesn't make any difference. Once the conceptual schema stops operating at the conceptual level--once it is incorporated into perception--then its implementation becomes among the givens in perception; and the study of the givens in perception, regardless of their alleged source, is a phenomenological study. Consideration of how these givens become givens, or any attempt to restrict them, removes the enterprise from the phenomenological field. It is thus erroneous to claim that phenomenology deals with the givens of perception only insofar as these are independent of learning. The consequence of such a position is that the organization of perception must be referred to something outside of perception--Köhler's neural fields, Husserl's quasi-Platonic pure ideas--in order to distinguish the 'truly givens' from the mere 'apparently givens'; and such a procedure, again, is not phenomenology. By contrast, looking at the physical movements that constitute the activity of a rat or a pigeon, and coming to see in those movements meanings--intimations of or approximations to the ultimate response that one wishes to shape--this, however strange it may seem, is phenomenology.

In this sense, therefore, despite the unfamiliarity and counter-intuitiveness of the claim, it is not unfair to judge that the practice of behaviourist psychology has, to varying degrees, long been based on unrecognized and covert phenomenological analyses¹⁰. The use of a covert phenomenological approach in the determination of the units of subject matter of experimental psychological investigations constitutes, I suggest, an outstanding example of a methodological-cum-philosophical strategy that is both implicit and successful, and that for some time was capable of being successful largely by virtue of being implicit. Descriptive analyses of behaviour of a covertly phenomenological sort have gradually grown sufficiently detailed and acute, particularly but not solely in some of the formulations of Skinner and his students, as to justify being described as a 'phenomenology of behaviour'. It is this gradual and covert development of a phenomenology of behaviour, I suggest, that constitutes the most important positive contribution of all the varieties of behaviourist experimental psychology to the future development of the discipline. In particular, to conclude with mention of the topic that introduced this discussion, it is the perceptual skills which many behaviourists have acquired in the course of their phenomenological training--skills in the identification of and subtle discriminations between responses as embodied in an ongoing stream of behaviour--that have equipped them to be at the forefront in developing and extending the techniques and the range of application of behaviour therapy and other forms of behaviour modification.

But while the inadvertent contribution of behaviourism and neobehaviourism to the development of psychology may thus have been great, and especially notable in the various fields of psychological

technology, that contribution is nevertheless, as a phenomenological contribution must almost always be, a propaedeutic one, not science but some of the groundwork on which science is built; and in psychology the science which could be built on that groundwork still lies an indeterminate distance away, in one of psychology's many possible futures. What behaviourism as the most important single influence in the continuing development of modern psychology can be said to have left us, besides the important negative lessons discussed earlier, is some portion of the tools appropriate for building a science--but not the science itself, and very little even in the way of durable preliminary structures which can be taken into the science.

Footnotes

Footnotes to Chapter 1.

1. It should be made clear that psychoanalytic theory fares no better than behaviourist theories at Peters' hands, and that this conclusion applies to both equally.
2. Cybernetics and information theory were developed independently of general systems theory, but are fully compatible with it and can be seen as particular applications of general systems principles. These two theories have, however, been applied extensively in the context of neobehaviourist theories, and in support of such theories. Von Bertalanffy (1971) considers that their application in such contexts is ill-advised, and that it reflects an artificial and arbitrary partitioning of the systems approach. Koch (1964, p. 32) makes a similar judgment, and deplores what he considers the unreasoned support given by information theorists to behaviourist theories.
3. These two considerations, that he is writing as a disillusioned behaviourist, and that he has no systematic alternative to offer, may account in part for the pessimistic title which he gives to a recent article: 'Psychology cannot be a coherent science' (Koch, 1969).
4. I am not competent to judge unequivocally whether Bergmann's position in this book should be considered incompatible with behaviourism or merely irrelevant to it. However, his emphasis on intensionalism (that is, on mental acts consisting in the referral of meaning to external objects) seem to make the relationship one of incompatibility, at least with most forms of behaviourism.
5. This 'physicalist' thesis was of course at the heart of the logical positivist support of behaviourism.

6. Palermo's views on the succession of scientific systems in psychology will be discussed in detail in Chapter 2.
7. Taylor (1964) provides further examples.

Hebb in the first chapter of his The Organization of Behaviour [1949] speaks interchangeably of 'animism' (the view that animate behaviour must be explained in terms of 'purpose') and 'interactionism' (the view that behaviour is the result of the interaction of observable physical and unobservable 'inner' or mental processes) and of course 'mysticism' (which doesn't seem to have a very clear sense in Hebb's usage but which means something counter-empirical, unscientific and generally nasty). Similarly, Spence [1944] speaks of animistic theories as those in which the relation of the (unobservable) constructs to the empirical (observable) variables is left entirely unspecified (Taylor, 1964, p. 8).

8. Behaviourism in its heyday never succeeded in its aim of altogether ridding psychology of mentalism in this general sense of course; psychoanalysis and gestalt psychology were two successful movements that were at times particularly objectionable. However, both of these movements earned a certain amount of (sometimes grudging) respect from behaviourists: psychoanalysis for its rigorous determinism and its tension-reduction models of human functioning (both acceptable to many behaviourists), and gestalt psychology for its physicalism and its carefully controlled laboratory studies. The new mentalism often has none of these saving graces.
9. Even in psycholinguistics, there are some signs that Chomsky's technical contributions are proving less generally applicable than was once hoped. The formalisms of his transformational grammar are proving increasingly difficult to relate to actual language use (e.g. d'Arcais & Levelt, 1970).
10. Book publishing in itself, as distinct from reports of current scholarly research, also reflects a change of mood. For instance, the phenomenological writings of Franz From have recently been

translated and published in English, twenty years after their first publication in Danish (From, 1971). At the other end of the mentalist spectrum, Titchener's Systematic Psychology: Prolegomena has recently been re-issued (Titchener, 1972).

11. Peters' description of Hull's interests is, admittedly, an overstatement. While Hull published little, if anything, on these complex matters, he was intensely interested in them and regularly discussed them in seminars that acquired for their participants a status similar to that of Pavlov's 'Wednesdays'. The cooperative volume Frustration and Aggression (Dollard et al., 1939) was composed largely under his inspiration.

12. There has been an interesting controversy over this model. The main opposition to Huttenlocher's model has been from Clark's model of syllogistic reasoning as dependent on semantic representations and purely linguistic operations. The significant feature of the controversy, in the present context, is that Clark's operations are as unabashedly mentalistic as are Huttenlocher's images, while both researchers are able to conduct their dispute by appeal to quite incontrovertible experimental data (for references and an overview see Huttenlocher & Higgins, 1971, 1972; Clark, 1969, 1972).

Footnotes to Chapter 2.

1. Kuhn writes:

...I was struck by the number and extent of the overt disagreements between social scientists about the nature of legitimate scientific problems and methods. Both history and acquaintance made me doubt that practitioners of the natural sciences possess firmer or more permanent answers to such questions than their colleagues in social science. Yet, somehow, the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the

role in scientific research of what I have since called 'paradigms'...Once that piece of my puzzle fell into place, a draft of this essay emerged rapidly (Kuhn, 1962, p. x).

2. Krantz (1965) attempted to elaborate on a suggestion of Kuhn's that a shift in the pattern of references in technical journal articles might index the imminence of a scientific revolution, but did not seriously examine or defend the appropriateness of considering psychology as a paradigm-based science. Katahn and Koplin (1968), likewise assuming the appropriateness of analysing psychology in terms of paradigms, tried to explain the controversy aroused by Breger's and McGaugh's (1965) analysis of behaviour therapy by showing how Breger and McGaugh were working from a different paradigm from that of their opponents. Numerous other respectful and sometimes wistful references to Kuhn's analysis have appeared in the literature, without any attempt at strict application of his thesis to psychology (e.g., Rychlak, 1972, quoted above; several mentions in Dixon & Horton, 1968). As indicated in the text, Palermo's papers were the first to attempt to show in detail the relevance of Kuhn's analysis to psychology; Palermo's thesis and analysis were independently paralleled, however, with somewhat different examples, by Segal and Lachman (1972).
3. Masterman (1970) has identified twenty-one distinguishable uses of the term 'paradigm' in Kuhn's treatise. She groups these into three distinct classes, but does not emphasize their continuity as increasingly distant abstractions from the exemplary achievement itself.
4. Warren is, however, somewhat vindicated on this point. The wide diversity of alternative approaches to psychology does make it difficult to see how behaviourism could count as a successor to Wundtian structuralism in an already mature, paradigmatic science. The first paradigm in a newly matured science may initially stand out as a

beacon in the midst of chaos, but one would expect that by the time the next paradigm came along the scientific discipline would have achieved some order.

5. As Duhem (1914) has shown, this is an invariable limitation of metaphysical principles, and recognition of the limitation helps to clarify the suggestive role that metaphysics can play in science. Duhem's point is generally accepted among those philosophers of science who concern themselves with metaphysics at all (cf. Watkins, 1956).

6. In a footnote, Briskman acknowledges that "the difference between paradigms and research programmes is not unambiguously clear (Briskman, 1972, p. 94)," but suggests two possible distinctions between them. The first is that research programmes need not be exclusive: there can be several competing research programmes in a science, whereas on Kuhn's original view there is almost invariably only one paradigm. The second is that paradigms have a central core of substantive content, functioning as exemplary solutions to model problems; research programmes on the other hand--and this point certainly applies to behaviourism--can be sufficiently characterized by general acceptance of a methodology. The first point may be useful in some contexts, but does not help to distinguish behaviourism as one research programme from behaviourism as one paradigm. The second point, so far as it concerns the lack of central defining content in behaviourism, is, I think, a good one. Behaviourism did not have a central core of scientific content. But behaviourism was very unusual in this regard, and its lack of any central content itself requires a special explanation. Behaviourism's non-contentual, almost exclusively methodological character cannot support a general demarcation of paradigms and research programmes

unless Briskman wishes to maintain, as he almost certainly does not, that scientific research programmes are typically devoid of central or defining content. Behaviourism's primarily methodological character thus not only requires a special explanation, it also serves as evidence against assimilating behaviourism to any general model of scientific progress (see pp. 68-81).

7. Briskman's 'research programmes' are not claimed to be entirely of his own invention; rather, as indicated in the text, they are taken with some modifications from the account given by Lakatos (1968, 1970). It may be thought, therefore, that justification for applying the concept of research programmes to science in general should properly be sought in Lakatos' writings. Such a procedure would be of little help in trying to characterize research programmes as general alternatives to paradigms, however. Lakatos' analysis was, it is true, originally intended to be a Popperian alternative to Kuhn. But in refining his position Lakatos seems to have moved so close to Kuhn on fundamental issues that it is very difficult to separate them on the basis of their differential predictions for or interpretations of scientific advance (see the cogent review by Bloor, 1971). The vagueness of Briskman's model specifically as an alternative interpretation of psychology is thus, if anything, increased by appeal to Lakatos. If there really is practically no difference between Kuhn's and Lakatos' position, then much the same historical evidence will be relevant to the question of whether either one or the other of them applies to behaviourism.

8. This article was written before either Warren's or Briskman's, and was accepted for publication before Palermo's extended statement of his analysis appeared (Palermo, 1971). However, because of differential

publication lags in different journals, it was published last. As a result, it considered only Palermo's brief summary statement of his position (Palermo, 1970).

9. It is not overly surprising, even without anticipating the argument, that it could have such features (e.g., accepted methodology, concentration of interest on a set of related problems) if it did not have a paradigm. Kuhn, after all, did not discover these features of scientific activity. Rather, these are some of the typical features of scientific activity that his account is intended partly to explain. It is of course true that Kuhn was for some time one of the few philosophers of science who stressed the significance of these features.

10. Autonomous, that is, again with respect to the criteria which it itself comes to make acceptable. It is obviously not autonomous with respect to all criteria, but the criteria by which it is assessed are far less restricted than those for the assessment of research based on it. For instance, it was not uncommon around the middle of the last century to proclaim that scientific explanation consisted in the reduction of observed phenomena to the principles of Newtonian mechanics (cf. the quotations from du Bois-Reymond and Helmholtz, footnote 7 to Chapter 3). This general criterion for assessment of physical research was of course not the one applied to Newton's own research. Those criteria included generality, mathematical elegance, and the applicability of Newton's theories to then current problems. Such general criteria are not totally devalued during a period of normal science, but are subordinated to the more specific paradigm-based ones, which have more unambiguous application. ('Application to current problems', of course, becomes itself one of the paradigm-based criteria.) Severe and longstanding incompatibility between the two kinds of criteria may

be an important feature of a situation in scientific research in which a crisis and revolution are imminent.

11. Palermo is by no means alone, however, in characterizing paradigms so minimally that almost anything could serve as one. Feyerabend (1970), for instance, says that as he sees it, organized crime, and particularly safecracking, could qualify as a paradigm-based science. Kuhn himself (1970a, 1970b) would now agree that the criteria for attribution of a paradigm could be far looser than those which he maintained in his 1962 treatise, and which I am maintaining here. In loosening these criteria, however, Kuhn also quite explicitly minimizes the functional significance of paradigms as providing the basis for research in a scientific field, where this latter concept is itself defined in terms of content, problems, etc. In Kuhn's revised thesis, which he has as yet presented only sketchily, paradigms function only in the context of scientific communities, defined in terms of patterns of mutual awareness and personal communication among their members. Paradigms in this revised form, however, could not begin to support the weight of explanation which Palermo loads on them here. It should go without saying that both Palermo and I are concerned in the present context only with the functional significance of Kuhn's 1962 paradigms.

Palermo is also, however, the co-author of a further paper (Weimer & Palermo, in press) which attempts to characterize behaviourism in terms of Kuhn's 1970 paradigms. The attempt may be somewhat premature, since Kuhn has not yet developed his new ideas beyond the level of a rough sketch. In general, however, if Kuhn's new analysis is indeed applicable to behaviourism, or to any other scientific field, it will necessarily be in an altogether different way than his original analysis was intended (by Palermo) to be. Kuhn's present interests

centre on the micro-structure of science, on analysis of working-group structure as revealed by who talks (or writes) to whom. Kuhn describes the minimum size of such groups as less than twenty-five persons. He makes no estimate of the maximum size, but it cannot be very large--large enough, say, to comprise 10% of the living psychologists or microbiologists--because of the rapidly increasing complexity in personal communication as a function of increase in group size. The working-groups or communities thus identified and studied may of course serve as opinion leaders to the rest of the practitioners in the discipline, and whatever differentiates such groups from other practitioners (communication patterns, ideas, general modus operandi--i.e., their own particular 'paradigm', as Kuhn now uses the term) may sometimes have great thematic significance. But then again, and at other times, it may not. Study of the patterns of small and medium-sized group communication is altogether separate from a study of the influence of the group or of the importance of what is communicated within the group.

Thus, while Weimer and Palermo may be quite in accord with Kuhn's present position when they write of paradigms in behaviourism--especially since they differentiate Tolmanian, Hullian, etc., paradigms and apply them to the conduct of the workers grouped around each major figure (cf. p. 76, above)--their analysis does not throw much light on the career of behaviourism as a general movement within psychology, as Palermo's original analysis attempted to do. The paradigms they describe have little or nothing to do with directing and justifying scientific inquiry or with ensuring scientific cohesiveness beyond the membership of a self-selected clique--such functions being what the original paradigms were expressly formulated as performing. If every group

doing anything has its paradigm, then paradigms have nothing intrinsically to do with reducing the proliferation of groups, and the question of the directedness and cohesiveness of scientific disciplines, or of major segments of them, requires an answer that has nothing to do with paradigms. But in fact, while analysis of intra-group communication patterns may be a valuable and worthwhile study in itself, Kuhn's 1970 paradigms have next to no connection with his 1962 paradigms, and each conception has practically no relevance to the problems elucidated by means of the other. It is simply misleading to describe both conceptions by the same name.

12. The brief exposition given here of the constitution of a paradigm is of course closely based on Kuhn's (1962) treatise, but contains a bit more than reportage. Masterman, in her sympathetic critique of Kuhn, writes:

... 'why', he asks himself (p. 11) is the paradigm, or scientific achievement, 'as a locus of professional commitment, prior to the various concepts, laws, theories, and points of view that may be abstracted from it?' Unfortunately (and typically), having posed this highly germane question, Kuhn gives himself no answer, and the reader is left to work out the answer for himself, if he can (Masterman, 1970, p. 66).

In fact, Kuhn does not leave the question quite so up in the air as Masterman claims (cf. Kuhn, 1962, Chapter 5: 'The priority of paradigms'), but he does leave much of the answer implicit and hence available as a source of confusion. The account given here is an attempt, but undoubtedly not the only possible one, to make it more explicit.

13. There were, it is true, some political influences which affected the development of psychology in Russia, both before and after the revolution, and which eventually were instrumental in establishing

the Pavlovian approach as predominant. These influences would qualify a Kuhnian analysis, but need not vitiate it, for two reasons. The first is that Kuhn's analysis does not exclude consideration of external pressures on the growth of a science, although such pressures are not treated in his book (cf. Kuhn, 1962, p. xii). The second, and more important, is that however much Pavlovian psychology may have been dependent on political factors to establish it as the dominant approach in later Soviet psychology, it has proved sufficiently viable there to retain its predominance on scientific grounds. It thus contrasts sharply with, say, Lysenkoist genetics, which became predominant on political grounds in the apparent absence of any scientific merit. For a review emphasizing the systematic character of modern Soviet psychology in relation to Pavlov, see Anokhin (1968). A more comprehensive synthesis of psychological theory, which draws heavily on Soviet research and again emphasizes the role of Pavlov's work in its systematic development, is made by Razran (1971).

14. Bolles (1967), summarizing Hilgard and Marquis (1940), notes:

...there was roughly a decade from about 1916, when Watson first promoted conditioning, to at least 1926, during which conditioning was accepted as a valid explanatory device and sometimes even proposed as the basis for all learning. All this time there was virtually no empirical support for the claims made for conditioning (Bolles, 1967, p. 317).

15. See Humphrey (1951) for detailed discussion of and references to the controversy. Humphrey argues persuasively that the discussion of meanings in Titchener's counter-experiments begs the question, and that it involves Titchener himself either in the stimulus error or in an unacknowledged dependence on an imageless, meaning-carrying component in thought. Furthermore, Mary Henle has shown that the stimulus error, as Titchener conceived it, was inescapably present in his treatment of

meaning in other contexts as well (Henle, 1971). Such results and analyses go far to show that Titchener's sensationalistic hypothesis was wrong, but not that it was meaningless or empirically sterile. If anything, they suggest the opposite; as Popper (1959) points out many times, the empirical status of a theory is rendered forever secure by the act of its refutation.

16. The repudiation of Wundt's distinction was expressed most forcefully by Kantor (1921).

17. Such, at least, was Tolman's intent in introducing intervening variables. However, as pointed out in Chapter 1, intervening variables are almost impossible to anchor unambiguously to behaviour. In his last theoretical statement, Tolman (1959, p. 148) acknowledged the insurmountable difficulties involved in any rigorous use of intervening variables in theory construction, and declared that "they are merely an aid to thinking ('my thinking' if you will)."

18. If purposes, expectations, etc., can be considered as mental entities, separate from the behaviour which they guide and direct, then the behaviour itself can be freely regarded as elementaristic and purposively neutral, without the intrinsic purposefulness revealed by the behaviour thereby being denied.

19. Spielberger and DeNike (1966), discussing "theorists in the Hull-Spence tradition", observe that

The model developed by these theorists was designed to account for the behavior phenomena exhibited by nonarticulate organisms or by humans in simple learning situations in which the operation of higher mental processes was minimal, for example, in eyelid conditioning and rote learning. A major difference between the views of descriptive i.e., Skinnerian behaviorists and those of Hull and Spence is that the latter never claimed that their theoretical concepts would hold for complex verbal processes. In his discussion of the postulates and methods of

behaviorism nearly 2 decades ago, Spence (1948, p. 76) noted that:

...in dealing with the more complex types of animal and human behavior, implicit emotional responses, covert verbal responses and not easily observable receptor-exposure and postural adjustments will have to be postulated...

and that:

It is in this realm of theorizing that the verbal reports of human subjects are likely to be of most use to the behavior theorist, for presumably these reports can be made the basis on which to postulate the occurrence of these inferred entities.

20. The term was coined by Dijksterhuis (1961), to satisfy the need for a noun corresponding to 'mechanistic'.

Footnotes to Chapter 3.

1. For discussion of this epistemological separation see Burt (1932) or the summary in Mackenzie and Mackenzie (1974).

2. For instance, elementarism and associationism in British psychology became temporarily unpopular with the rise to dominance of the Scottish 'common sense' school of Reid and Stewart. This development in psychology paralleled the temporary decline in popularity of physical atomism in Scottish chemistry, under the influence of Joseph Black. Physical atomism was revived and extended in chemistry shortly after the publication of Dalton's atomic theory in 1804, and the corresponding psychological doctrine was revived in an extreme form by James Mill, with the publication of his Analysis of the Phenomena of the Human Mind, in 1829. The correspondence is, at the very least, suggestive, although in these instances it would be difficult to document any claim of direct influence.

3. Frank (1949a) points out that Mach's position has often been treated by philosophical critics as equivalent to Berkeleyian idealism, to the position that perceptions are the only real existents and hence that

reality has a purely mental character. Such an interpretation is evidently unfair to Mach. His claim was that perceptions are, not all of reality, but all that is available to us; thus, talk of anything existing independent of perception is empirically meaningless. The motto for Mach's position would not be Berkeley's esse est percipi--to be is to be perceived; but rather cognoscendi est percipi--to be known is to be perceived. The interpretation of Mach's analysis as idealistic or, at best, psychologistic, seemed to be the main basis for the rejection of Mach in the implementation of positivism as an explicit programme for psychology (cf. Stevens, 1939).

4. It can be seen that this positivist repudiation of metaphysics would tend to foster impatience with the subtle distinctions between alternative metaphysical systems, since they are all equally meaningless; or at least it would do so for those positivists whose concern with metaphysics was purely negative (i.e., unlike Duhem). In dealing with metaphysical positions, therefore, anti-metaphysical positivists might be expected to choose the simplest, or most clearly expressed, or (for the sake of emphasis) most extreme of a set of metaphysical formulations and make it stand, and fall, for the whole set. Carnap (1932), for instance, singles out Heidegger's undeniably obtuse analyses of Being as representative of all speculative metaphysics, and shows the empirical emptiness of Heidegger's statements rather easily.

5. This empirically unwarrantable character of the affirmation of the truth or falsity of a scientific system thus proceeds, in the present instance, from the non- or trans-empirical character of reality as defined. It will be shown in detail later that this unwarrantable character of the affirmation follows also, although relatedly, from the

limitations of logic when applied to the interpretation and characterization of phenomena.

6. Cf. Einstein's aphorism, "God is subtle, but He is not malicious," and his biographer's comment:

With these words he was to crystallize his view that complex though the laws of nature might be, difficult though they were to understand, they were yet understandable by human reason (Clark, 1972, p. 38).

Einstein's conception of the world was complex however, if not contradictory, and he cannot be cited as an unequivocal exponent of any view of science. As Frank comments:

Roughly speaking, we may distinguish, according to Max Planck, two conflicting conceptions in the philosophy of science: the metaphysical and the positivistic conception. Each of these regards Einstein as its chief advocate and most distinguished witness (Frank, 1949b, p. 271).

7. For example, in 1847 Hermann Helmholtz, in his classic paper 'On the conservation of force'--the paper in which he promulgated the conservation principle--stated:

The task of physical science is finally to reduce all phenomena to forces of attraction and repulsion the intensity of which is dependent only upon the mutual distance of material bodies. Only if this problem is solved are we sure that nature is conceivable (quoted in Frank, 1949a, p. 213).

Similarly, Emil du Bois-Reymond, a founding member with Helmholtz of the heavily materialistic Berlin Physical Society, observed in 1872 in his (at the time) equally famous paper, 'On the limitations of natural science':

The cognition of nature is the reduction of changes in the material world to the motions of atoms, acted upon by central forces, independent of time...It is a psychological fact of experience that wherever such a reduction is successfully carried through our need for causality feels satisfied for the time being (quoted in Frank, 1949a, p. 213).

Frank, who strongly disapproves of any such enshrining of scientific

theories, comments on these two passages:

Is this not an amazing fact in the history of the human mind? As Newton set up his theory the introduction of the central forces of attraction was regarded as a particularly weak point of this theory. It was accused of requiring the introduction of an element that is philosophically absurd. But what happened about a hundred years later? It was claimed as a "psychologic fact" that just the same thing--the reduction of a group of phenomena to the action of central forces--satisfies our need for causal understanding. And the derivation of physical theorems from the action of these forces, which were formerly condemned as unconceivable, was now the guarantee that nature is conceivable (Frank, 1949a, pp. 213-214).

8. In his unauthorized introduction to Copernicus' treatise on the heliocentric hypothesis; see Koestler (1959) for discussion.

9. Newton's disclaimer, made in his Opticks, is well known:

To tell us that every species of thing is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing: But to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from those manifest properties, would be a very great step in philosophy, though the causes of those principles were not yet discovered: and therefore I scruple not, to propose the principles of motion above-mentioned, they being of very general extent, and leave their causes to be found out (quoted in Burt, 1932, p. 219).

10. Others, such as Leibniz, remained unconvinced to the end, raising substantive objections to Newton's fundamental concepts that, in retrospect, seem prophetic (see Koyré, 1957, for discussion).

11. The putative impossibility of an experimental application of introspection (founded on the ineffability of the ego) was the basis on which Kant rejected the possibility of a scientific psychology. It has been conjectured (by Turner, 1967) that Kant might therefore have had no objection to a psychology based strictly on the study of behaviour. It is an interesting conjecture, and may well be correct; it seems likely, however, that Kant might have had severe doubts about

how much such a science could accomplish, doubts amounting perhaps to a conviction of its triviality.

Footnotes to Chapter 4.

1. It would be well to differentiate here at least three possible meanings of 'objective'. Two of them were mentioned briefly in Chapter 2. In the first sense, 'objective' means 'free from bias' or 'concerned with observable events, whatever they might be'; the antithesis is 'subjective', meaning 'biased'. In the second sense, less easy to define, 'objective' refers to the use of observational and logical methods supposedly derived from physics, with a corollary that any phenomena not amenable to investigation with these methods are ephemeral and not a proper subject of science. This is the sense that I dubbed 'objectivist' in Chapter 2; the antithesis is 'subjective' or 'mentalistic', here meaning 'concerned with unobservable inner entities or processes', and is approximately synonymous with 'mystical' (cf. footnote 7 to Chapter 1). In the third sense, 'objective' means 'external to the perceiving organism' or 'publically observable'; the antithesis is again 'subjective', this time meaning 'dependent on the private experience of the observer'. This third sense is the only one among the three given which is merely descriptive, that is, in which 'objective' and 'subjective' are not terms of approval and abuse respectively. In this third, descriptive sense, all the introspective methods are subjective. Unless otherwise stated, 'objective' and 'subjective' will be used in this descriptive sense throughout the present chapter. If the first, evaluative sense is overlaid on the third, descriptive sense, the result is the second, which purports to be both evaluative and descriptive.

2. It should be clear that the introspective psychologies of, say,

Wundt, Külpe, and Titchener, were not guilty of this fallacy, at least so far as they were confined to the study of human introspective psychology. The introspection carried out by these psychologists was anything but casual and, more important, was not assumed to provide any easy, immediate, or incontrovertible information about the mind's contents and operations.

3. Boring underscores this point, observing that "Romanes lacked a satisfactory classification of human faculties with which to work. He was thrown back upon Locke and the associationists for his terms (Boring, 1950a, p. 474)."

4. The claim that we all make use of the analogy in inferring consciousness in other persons is another of those hangovers from Cartesian philosophy for which Descartes cannot be held wholly responsible. In practice, it seems to be rarely if ever true that we infer consciousness in this way. It is more likely the case that, except when we are acting as philosophers, we do not rationally infer consciousness or sentience in other persons at all, so much as uncritically assume it in the course of the socialization process--although it is certainly something less sophisticated than the consciousness of the philosophers that we assume by these means. This insight was reached independently by Comte, Wittgenstein, G. H. Mead, and (in an elliptical fashion) Skinner. It is not, however, an insight that can even yet command universal acceptance, and certainly not one that we can fault Romanes for lacking.

5. This minimization of the experimenter's subjectivity as an analytical tool has no bearing, of course, on the status or the resolution of such questions as whether other organisms are conscious, and if so which ones, and what their mental lives are like, etc.--questions which the analogy was supposed to be used to answer, but which in fact it was

incapable of answering. It is often claimed that these questions are meaningless. They may or may not be, but in any case the point to be established here is simply that they cannot be resolved through application of the analogy. Whether there are any other methods that could resolve them is certainly dubious, but that is only to say that it is questionable. This entire discussion, however, might seem otiose to those who have rejected any consideration of subjectivity, and who are thus satisfied with rejecting these questions as meaningless and with rejecting the method of analogical inference as a misguided attempt to answer meaningless questions. Here the point is that such rejection in principle is by no means universally accepted among psychologists, and is becoming less widespread (cf. Chapter 1); that such an in principle rejection cannot itself be rigorously justified (cf. Chapter 5); and that it is not in any case necessary as a basis on which to reject Romanes' specific kind of concern with the subjective experience of other species, as the present discussion attempts to show at length.

6. Neither of these examples is taken from Romanes' writings. Rather, they refer to the later controversies in behaviourist psychology over transposition and cognitive maps, respectively.

7. On this matter see also footnote 9 to this chapter, below. Romanes' analysis was particularly weak on this point, that is, in the establishment of just what a given piece of behaviour could serve as evidence for. It is equally weak on the matter of accurate observation of the behaviours themselves. Both of these limitations follow chiefly from his almost total reliance on anecdotal reports, and together they render his particular findings of little lasting scientific value. I have not otherwise criticized Romanes' work for its reliance on anecdotes,

although this is the criticism most often brought against it. The point is that no such criticism is really necessary, once the minimal value of Romanes' data has been initially established. Romanes was well aware of the dangers inherent in the anecdotal method, which was in deservedly poor repute at the time he was writing, and he tried to take great care not to fall into the habits of the "anecdote mongers". Nevertheless, he felt that if he applied the strictest available criteria of selectivity to such reports as reached him, the resulting data would be of at least some use in suggesting evolutionary trends, even if many or most of the particular conclusions had to be revised following detailed experimental investigation. While the worth of Romanes' data was even less than he expected, he was surely right, in this case, in making do with the best materials to hand. The value of his contribution never lay in the supposed accuracy of the behavioural observations which he compiled; rather, as he realized, it lay in the insights suggested by, and the further research stimulated by, his systematization of the currently available information and misinformation. Furthermore, it may be noted that despite the poor quality of Romanes' reportage by our standards, it was considerably more cautious and accurate than any other such compilations to that date, or indeed until several years later.

8. Yet another reason for insisting that the mental or psychic inferenda be specified independent of subjective experience is that otherwise they will be dependent solely on the subjectivity, not even of mankind in general, but of the individual experimenter. Since different persons differ in regard to their assimilation of experience, their characteristic ways of solving problems, etc., any attribution of mind based solely on reflection on one's personal consciousness would amount to

universalizing one's individuality, and thus to making personality differences the chief source of theoretical differences. We may suspect that differences in theory are sometimes dependent on differences of personality in any case of course, but it is not a tendency which we would usually want to institutionalize.

9. Besides the general desire for scientific rigour, and the need to counter the still prevalent tendency to anthropomorphism in the interpretation of animal behaviour, there was a theoretical basis for the canon. If "higher psychical faculties" evolve from lower ones, then on evolutionary principles they must become incorporated into the species as a result of environmental pressures which render them of some use, that is, which render them of selective advantage or survival value to the organisms which possess them. Now, there are doubtless many activities (responding differentially to written or verbal commands to "stop" or "go" for example) which can be controlled by either higher or lower faculties, that is, in this case, either on the basis of linguistic competence or on that of reinforced stimulus discrimination. If we see an animal performing such an activity, we cannot tell whether the activity is being controlled by higher or lower faculties. We can, however, judge unequivocally that the necessity for performance of this specific activity was never, in the evolutionary history of the species, the occasion for development or assimilation of the higher faculties, since ex hypothesi the lower ones can control it perfectly well. Development of higher faculties in this connection would consequently have been of no selective advantage to the organism; therefore, if they were to develop, they could be expected to be swamped in the genetic pool of the species. Thus, performance of such an activity, however it is governed in a specific instance, cannot serve as evidence for the

operation of the higher faculties even on the most generous interpretation; for had it not been necessary for the animal to perform some other activity unmanageable by the lower faculties the higher faculty in question would presumably never have evolved.

10. Hobhouse receives about a dozen incidental references in the three enormous and comprehensive volumes of Comparative Psychology by Warden, Jenkins, and Warner (1935), with almost no discussion of his findings or theories. His work is not mentioned at all in what remains a standard text, the Principles of Animal Psychology by Maier and Schneirla (1935). It is given one incidental reference in Boring's History (1929; unchanged in the 1950 edition).

11. It may be recalled that Titchener, after studying with Wundt at Leipzig, could not secure any position in experimental psychology in Britain, and was therefore forced to go to what he considered the colonies--Cornell--to gain employment. Titchener, of course, played only a negative role in the transformation of American psychology from a Wundtian to a functionalist stance.

12. James wrote:

The dilemma in regard to the nervous system seems, in short, to be of the following kind. We may construct one which will react infallibly and certainly, but it will then be capable of reacting to very few changes in the environment--it will fail to be adapted to the rest. We may, on the other hand, construct a nervous system potentially adapted to respond to an infinite variety of minute features in the situation; but its fallibility will then be as great as its elaboration. We can never be sure that its equilibrium will be upset in the appropriate direction. In short, a high brain may do many things, and may do each of them at a very slight hint. But its hair-trigger organization makes of it a happy-go-lucky, hit-or-miss affair. It is likely to do the crazy as the sane thing at any given moment. A low brain does few things, and in doing them perfectly forfeits all other use...Now let consciousness only be what it seems to itself, and it will help an instable brain to compass its proper ends.

The movements of the brain per se yield the means of attaining these ends mechanically, but only out of a lot of other ends, if so they may be called, which are not the proper ones of the animal, but often quite opposed. The brain is an instrument of possibilities, but of no certainties. But the consciousness, with its own ends present to it, and knowing also well which possibilities lead thereto and which away, will, if endowed with causal efficacy, reinforce the favorable possibilities and repress the unfavorable or indifferent ones (James, 1890, I, pp. 140-142).

13. Again, it was not totally without opposition that this equation was made, or that the consciousness of introspective psychology was retained in functionalism. Dewey (1896) stressed the complete integration of consciousness with stimulation from the environment on the one hand and with adaptive responses on the other. Experience, he maintained, could not be characterized outside the context of an individual's needs and actions; equally, 'stimulus' and 'response' could not be characterized independently of each other. Stimulus and response comprised a single integrated series, while consciousness was not something separate which integrated them, but rather that which established the integration as unique to the requirements of a specific individual. Dewey was thus attempting in part to overcome the subject-object distinction basic to much of Western thought, by arguing that before our world-view becomes shaped by philosophical abstractions, objects are given in perception primarily in terms of their relevance or relation to the perceiver, rather than as independent existents. There is some small irony in that Dewey's paper is sometimes cited as the founding of functionalism (e.g., by Boring, 1950a). In fact, while Dewey had considerable influence on the movement, largely through his personal influence on Angell, and while his 1896 paper certainly helped draw attention to considerations of biological functioning in consciousness, his principal efforts in the paper to reconstitute the

notions of stimulus and response had almost no systematic influence.

14. It may seem arbitrary to read Thorndike out of the functionalist camp, and in a sense it is; it is arbitrary to put him anywhere.

Thorndike was a sufficiently individualistic thinker that it is difficult to categorize him at all; Watson was disinclined to let him count as a behaviourist either. Assimilating him to the British movement in comparative psychology is purely a matter of convenience, due to the thematic and conceptual similarities that have been discussed. Thorndike eventually came to describe himself as a 'connectionist' (Thorndike, 1949), partly, one suspects, because the term had not been appropriated by anybody else.

15. There was also a direct thematic relationship between experimental introspection and the kind of comparative psychology characterized by the exclusive employment of subjective inferences, although the relationship was not a very extensive one. Wundt (see footnote 17 to this chapter) made systematic attempts to interpret the conscious experience of animals by means of the analogy, although he did not regard the endeavour as rigorously scientific. Titchener was never actively engaged in comparative research, but gave his blessing to such work so long as it was based on use of the analogy (Titchener, 1914). Titchener's student, Washburn, vigorously promoted an introspective type of comparative psychology as late as 1936. In her compendium of research on The Animal Mind, Washburn first made the standard disclaimer concerning use of the analogy, a disclaimer notable only in that it was more emphatic than any made previously by the defenders of the analogy.

Whether...our inferences are made on the basis of words or of actions, they are all necessarily made on the hypothesis that human minds are built on the same pattern, that what a given word or action would mean for my mind, this it means also for my neighbour's mind.

If this hypothesis be uncertain when applied to our fellow human beings, it fails us utterly when we turn to the lower animals. If my neighbour's mind is a mystery to me, how great is the mystery which looks out of the eyes of a dog, and how insoluble the problem presented by the mind of an invertebrate animal, an ant or a spider (Washburn, 1936, p. 2).

She then concluded, as Romanes had done, that however dubious any inferences based on the analogy might be, they had to be better than the alternative, which was to say nothing at all; and launched a few pages later into a serious discussion of the subjective experience of amoeba.

16. It is mere speculation, but it is possible that functionalist comparative psychology might have taken a different course if Dewey's attempt to reconstruct the subject-object relation had been more successful (cf. footnote 13 to this chapter). The fundamental point of his analysis, again, was that the constitution of a physical event as a stimulus is dependent on the organism that it is a stimulus for. The stimulus-as-perceived is thus not a function simply of its physical properties and of the sensory apparatus of the percipient, but equally and codeterminately of the motivations, life-style, and ecological niche of the percipient. We are inclined today to treat such determinants of perception as distortions of the information embodied in the stimulus event, but they could equally be regarded as merely comprising part of the relationship between physical event and percipient, no less than the percipient's particular sensory apparatus does. The effect that Dewey's analysis might conceivably have had would have been to obviate the sensationalism, and all that went with it, in functionalist comparative psychology by directing attention to the environment as it exists for the organism rather than as existing independently and being reflected, well or poorly, in the animal's consciousness. Such a

development, had it occurred, would almost certainly have been interesting. It would probably have been nearly as unlike the trends in British comparative psychology as discussed here, as it would have been unlike what actually happened on the American scene. It would perhaps have been most comparable with the kind of study on the umwelt, or perceptual world, carried out by von Uexküll (e.g., 1926, 1957).

17. Wundt's Lectures on Human and Animal Psychology (1892) contained analyses of the ideas and actions of animals which were similar in some respects to Small's, and, for that matter, to Romanes', but considerably more fanciful than either. He dwelled at length, for instance, on the reasoning processes in spiders. His account was not, however, so sensationalistic as Small's, in that it made extensive use of what has been called here subjective inferences to mental operations. The result, unfortunately, was so wildly speculative that Wundt's analyses came to be cited as cautionary tales, illustrating the fate of those who ignored the canon of parsimony (e.g., by Washburn, 1936).

18. Watson's 1907 paper was the last in which he made any inferences to subjective experiences. His polemical 1913 paper was based on lectures which he had delivered the previous year.

19. One could speculate on the distant influence of faculty psychology on the development of comparative psychology in Britain, in keeping such a 'faculty' conception of mind available for consideration. There is nothing inherently unscientific about faculty psychology--Guilford's work on the structure of the intellect is faculty psychology--unless it is combined with the nominalist fallacy that to name something is to explain it. That faculty psychology had a continuing influence in

British psychology--through Gall, Spencer, and Bain--has been shown by R. M. Young (1970).

20. In particular, the faculty conception of mind, which was quite widespread in American pedagogy before the introduction of the introspective 'new psychology', was employed almost exclusively for the inculcation of moral and religious precepts.

21. The irony would have been even more pointed if the British approach to comparative psychology had continued to develop and to stimulate further progress in the field. Unfortunately, it did not do so, for reasons that are complex and not altogether clear. The main one seems to be that there never developed an autonomous scientific tradition of comparative psychology in Britain. The writers mentioned, and others, all wrote comparative psychology as part of an overall evolutionary synthesis, or in order to propound general principles that would have applicability to social philosophy, ethics, metaphysics, etc. To a surprising and commendable extent, such general considerations were kept out of the comparative psychology writings themselves; but when their cogency faded after the widespread acceptance of Darwinian principles they did not come to be replaced by purely scientific considerations as a basis on which to do research in the field. The general lack of support for experimental psychology in Britain at the time is doubtless implicated in the failure of an independent scientific tradition to develop specifically in comparative psychology.

22. James' speculations on the selective functions of consciousness, as quoted in footnote 12 to this chapter, are a general kind of subjective inference to mental operations. They were of such a sort, therefore, that they could not be taken up and elaborated in the

context of 'objectified' experimentation on animal consciousness performed by means of minimal subjective inferences.

23. Behaviourism thus was deterministic from the beginning, but it implied a determinism of environmental events, as did psychoanalysis, rather than of physical processes. The ambivalence in later behaviourist theorizing about physiological reductive explanation stems in part from this.

24. Angell (1913) testified to the difference between comparative psychology and the rest of psychology in this respect, in a paper written independently of Watson's (1913a) polemic. He judged that introspection and introspective formulations might eventually have to be dropped from comparative psychology, but thought that it would be very unwise to eliminate them from human psychology.

25. For the sake of retaining some perspective in the face of this kind of argument, we may remind ourselves that neither of Watson's claims stands up to examination--the physical sciences are neither so reliable, nor the introspective psychological ones so unreliable, as he maintained. For the extent and limitations of inter-experimenter reliability in introspective psychology, see the brief discussion in Chapter 2, above, and the longer one in Humphrey (1951) on which it is based. For the highly comparable extent and limitations of inter-experimenter reliability in the physical sciences, and the ad hominem explanations sometimes offered when replicability does not obtain, see Kuhn (1962) or Polanyi (1958). Watson's claim on behalf of physics is, of course, quite consonant with and typical of a positivist or otherwise methodological conception of science.

26. Watson's argument was in this context directed specifically against that version of the phylogenetic continuity hypothesis that sought to

lift animals up (by maintaining that they have nascent higher faculties) rather than to pull man down (by maintaining that he has only highly developed lower faculties). The more general application of the argument to instincts is fairly apparent however, and was made by Watson himself (e.g., 1924, Chapter 5) as well as, more notoriously, by his student Kuo (e.g., 1924).

27. Herrnstein (1972) has shown in some detail how the conception of 'instincts' became transformed into that of 'drives' throughout the course of behaviourism.

Footnotes to Chapter 5.

1. As the quotation indicates, Cattell's statement reflects already established laboratory practice, but Woodworth (1931) affirms that it was the first attempt, so far as he could determine, to justify such practice systematically.

2. It was, of course, not just neobehaviourism that was affected, but also, and by extension, other approaches to psychology as well. Lewin's (1936, 1938) attempts to construct a formal (and often hypothetico-deductive) field-theoretical account of behaviour while remaining firmly within a gestalt orientation provide what is perhaps the most notable example. But while the 'new view' of science and the hypothetico-deductive prescription had some considerable influence throughout psychology, it was specifically within neobehaviourism that they had their greatest influence and that their promise was most eagerly taken up. Furthermore, it was largely due to their fervent advocacy by neobehaviourists that these principles came to have such a widespread influence; it is primarily in this respect that behaviourism may be said to have established the standards and models for theorizing throughout psychology.

3. It is worth pointing out that the sarcasm is not at all justified. The logical positivist principles and techniques adopted within neobehaviourism, for the fulfillment of the scientific programme originally enunciated within classical behaviourism, were certainly highly abstract and rarefied, and had at times only a tenuous contact with actual scientific practice; but such a judgment can often be made about the theoretical and methodological apparatus, whether it works or not, of any advanced science. Even on the basis of its goals as Koch describes them, neobehaviourism can be seen as the only systematic, extensive, and detailed attempt ever made to fulfill Leibniz' dream of a universal calculus; to fulfill it, furthermore, in the context of ongoing scientific inquiry, where if accomplished it could do most good, rather than in that of the 'rational reconstruction' of an already completed theoretical structure, where in the absence of extension to just such ongoing inquiry, it could be little more than ornamentative; and to fulfill it, finally, through a sophisticated combination of logical and empirical operations and manipulations, so that it could be applied to the characterization of the world of contingent facts without impugning their contingent and independent character. Had the attempt been successful, the 'strange expectation' which Koch speaks of, that the extent to which a hypothesis was intuitively acceptable (i.e., 'plausible') was of little consequence, would have been perfectly valid. The failure of the attempt, both in practice and in principle, may stimulate a reappraisal of the limits of scientific technique, may perhaps provide the basis for some further enlightenment, and may occasion some sense of wonder; but it seems odd to take it as grounds for our scorn.

4. It may be taken as a rule of thumb that, in an age in which

scientific theories are generally thought of as being 'materialistic', the mark of a sincerely positivist orientation is the repudiation of the absolute status of materialist, as well as of idealist, theories. Thus Frank (1949a) characterized materialistic physical theories (such as those of Helmholtz and du Bois-Reymond) and scholastic philosophy as jointly and equally constituting the principal conceptual excesses which logical positivism was pledged to combat. Similarly, Hull (1937) criticized the behaviourist A. P. Weiss and the physicist Arthur Eddington equally, the former for his faith in reductive materialism, the latter for his faith in transcendental idealism--and both for their faith, as being the basis on which they each maintained adherence to their preferred positions to an extent greater than could be justified on rigorously scientific grounds.

5. It should be clear, and if not it should be made clear, that the examination made here and in Chapter 3 makes no pretence of being either a complete or a definitive critique of positivism in general or of logical positivism and related movements in particular. The aim is much more modest, although significant enough: it is to assess the potential adequacy, for the specific purposes for which they were initially designed, of the kinds of decision procedures developed within such movements. Those initial purposes were a) the ensuring of the rigorously empirical character of scientific statements, concepts, and theories, b) the construction of rigorous procedures for testing the validity (as well, that is, as the empirical status) of such statements, etc., and c) derivatively, but most importantly, the synthesis of these procedures into a completed logical framework which would be adequate to characterize and hence to regulate scientific inquiry. Thus, the analysis made here has nothing to say about the

internal constitution or philosophical merit of any of the positivist formulations to be considered, except insofar as these considerations are specifically relevant to the extrinsic task addressed by such formulations. Similarly, it is only formulations which are, or which were designed to be, applicable to the analysis and regulation of scientific inquiry which will be mentioned at all; as a result, the bulk of the most important contributions of the positivist tradition to logic, mathematics, and philosophy will receive no mention.

These considerations may serve to mitigate somewhat the presumptiveness involved in attempting to assess in just over 50 pages what is, after all, a dominant movement in the contemporary philosophy of science. What makes the enterprise possible at all is that for the most part it is not original. Most of the logical considerations to be adduced here are merely cited from earlier analyses. Many of the specific implications for the conduct of scientific practice are likewise borrowed from previous work. The relationship of the analysis given here, and of the point of view which it expresses and which has informed this monograph throughout, to earlier work in the history and philosophy of science, is described later. What is in any way original in the present analysis, or sketch of an analysis, is the way in which it attempts to systematize the various findings in terms of their overall implications for the practice of science and for the relationship of methodological analyses in general to such practice.

6. Cf. Kolakowski's formulation of this same requirement, as stated in Chapter 3, stipulating that a theoretical system incorporating abstractions

...must also be such that we do not forget that these abstractions are no more or less than means, human creations that serve to organise experience but that are not entitled to lay claim to any separate existence (Kolakowski, 1972, p. 15).

7. The conclusion to Hume's Enquiry Concerning Human Understanding (1777) states:

When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume; of divinity or school metaphysics, for instance; let us ask, Does it contain any abstract reasoning concerning quantity or number? No. Does it contain any experimental reasoning concerning matter of fact and existence? No. Commit it then to the flames: for it can contain nothing but sophistry and illusion (Hume, 1777; Selby-Bigge ed., p. 165; italics in the original).

8. Logical positivists, who at least initially tended to prefer a verificationist approach, were inclined to talk about empiricist criteria of meaning. Falsificationists, taking their lead from Popper (1934), preferred to talk about criteria of demarcation between science and other forms of quite possibly meaningful discourse. The difference does not really amount to much however, since as already shown any empiricist criterion of meaning is stipulative, hence conventionalist, hence no more than demarcative. Popper's criterion of demarcation is thus not different in kind from logical positivist criteria of meaning; it is just more accurately named.
9. The point was made even better by the Scottish chemist Joseph Black in 1803: "A nice adaptation of conditions will make almost any hypothesis agree with the phenomena. This will please the imagination but does not advance our understanding (quoted in Popper, 1959, p. 82)."
10. Duhem (1914) found it necessary to appeal to the eventual 'good sense' of scientists as the basis for choosing between theories.

The day arrives when good sense comes out so clearly in favor of one of the two sides that the other side gives up the struggle even though pure logic would not forbid its continuation (Duhem, 1914, p. 218).

11. In his most recent book, Popper (1972) adopts a kind of loose Platonic realism. In his most influential writings on the philosophy of science (Popper, 1959), however, his commitment to epistemological

realism was hardly at all reflected in his logical and methodological analyses. He now explains his diffidence as being due to his uncertainty regarding the coherence of a consistently realistic outlook, an uncertainty that was finally resolved for him upon gaining a more detailed understanding of Tarski's theory of truth.

12. Popper's considerable influence among some scientists is based on the belief that the logical asymmetry between induction and deduction renders conclusive disproof possible, so that the adoption of falsificationism makes it possible to achieve sure and certain knowledge of what is not the case, at least (e.g., Medawar, 1967). Such a belief rests on a basic misinterpretation of Popper. There are at least three reasons why knowledge gained through empirical and logical operations is not incontrovertible. The first is the logical invalidity of induction. The second is the availability of conventionalist or ad hoc stratagems for 'immunizing' a theory against disproof. The third is the relativity of concepts: observations cannot refute a theory, only statements based on them can; but statements embody concepts and concepts exemplify theories or world-views; hence theories of a highly general sort at least are among the determinants of observation-statements. The asymmetry of induction and deduction resolves only the first of these. The other two can be dealt with, Popper maintains, but only stipulatively, and thus never absolutely.

13. Strictly speaking, what is assumed is not invariance but independence, i.e., that changes in the value of the operationally defined property or concept are independent of and unrelated to changes in the values of the undesignated 'background' variables. This qualification does not affect the argument.

14. Bridgman's claim was that:

Reflection on the situation after the event shows that it should not have needed the new experimental facts which led to relativity to convince us of the inadequacy of our previous concepts, but that a sufficiently shrewd analysis should have prepared us for at least the possibility of what Einstein did...We should now make it our business to understand so thoroughly the character of our permanent mental relations to nature that another change in our attitude, such as that due to Einstein, shall be forever impossible (Bridgman, 1927, pp. 1-2).

15. Much the same point has been elegantly made by Seligman (1969) in the context of psychological experimentation, making use of more examples and less analysis.

16. This is not to deny that gratuities can sometimes be valuable, that a random selection among possibilities may at times disclose a relevant variable. The point is simply that while we may welcome the effects of chance when they are favourable, we do not depend on chance as the sole or the main source of our hypotheses.

17. This recourse to operational definitions may seem to contradict what was said earlier on the subject, concerning the subordination of operational analysis to theoretical considerations in the context of construction. It is perfectly true that the choice of variables and limits with respect to which the operational analysis is carried out is no more inherent in the problem situation within the context of reconstruction that it is anywhere else, nor is the analysis necessarily any more complete in this context. The point here is only that the content of the old theory need not place any limitations on what variables and limits can be considered significant to emphasize in the context of reconstruction. The basis on which the choice is made is irrelevant to the significance and import of the resulting analysis, but the variables and limits selected can be expected to be arbitrary with respect to--not entailed by--the old theory, since otherwise they would already have been emphasized in the context of construction.

18. Cf. Planck's gloomy conclusion that "New scientific truth does not triumph by convincing its opponents and making them see the light but rather because its opponents eventually die (quoted in Boring, 1963, p. 247)."

19. In fact, the present account bears close comparison with Kuhn's, and many of the basic ideas advanced here can be mapped onto Kuhn's analysis. The 'fundamental insights' exemplified by a theory correspond, at least roughly, to a 'paradigm', the 'context of construction' to periods of 'normal science', the 'context of reconstruction' to periods of 'crisis' and 'revolution', etc. Both accounts, furthermore, come to the same conclusion regarding the tenability of the distinction between the contexts of discovery and justification. There are many minor differences between the two accounts, concerning, for instance, the magnitude of the shift in attention necessary to constitute the beginning of a revolution or of the context of reconstruction, respectively. These are not fundamental, but I would not attempt to extend the present account of scientific cycles to the fine structure of science in the way that Kuhn does in his analysis. Furthermore, the present account, focusing as it does on the relationships between scientists and the contents of scientific disciplines, does not imply the uniqueness or singularity of the focus of commitment (theory, paradigm) so strongly as does Kuhn's.

The most important difference between Kuhn's account and mine, however, concerns the level of operation of the factors responsible for paradigm- or context-shifts. Even here, the difference is mainly one of emphasis. A persistent misinterpretation of Kuhn's analysis has been that his account of normal science and of scientific revolutions makes scientific progress dependent in large part on mob psychology

(Lakatos, 1970), irrationalism (Feyerabend, 1970), or catastrophism (Toulmin, 1970). These charges are levied because the factors which Kuhn cites as constitutive of the progress of science (paradigms, puzzle-solving, etc.) are not clearly operative at the level at which scientists make scientific decisions; that is, they are not clearly related to the factors ostensibly involved in such decisions. Such factors appear, at least to some critics, to be superordinate causal factors, independent of the methodological canons of science, shaping the destiny of science and scientists in a way over which the scientists themselves, acting as scientists, have little or no control (see Toulmin, 1970, for the clearest statement of this charge). Kuhn (1970b) has abjured and refuted the charges at length. It may be admitted, nevertheless, that his account sometimes gives at least the impression of positing such autonomous forces operating independently according to a kind of Hegelian dialectic--and that although this impression is mistaken it is one that can be avoided only with considerable care and attention to details. Furthermore, what has most attracted attention to Kuhn's account is its wide-ranging and broadly integrative historical sweep; the basis for its appeal is thus not highly conducive to a focus on details (this consideration does not, admittedly, provide an explanation for misinterpretations made by professional historians and philosophers of science).

The present account may serve as a complement to Kuhn's in this one respect, by emphasizing what might loosely be called the 'semantics' of scientific research (its relation to 'things') in contrast to its sociology. The factors which necessitate commitment to scientific realism (cf. 'acceptance of a paradigm') in the context of construction directly arise from and relate to the exigencies associated with

interpreting and selecting empirical data and relating them to a theory. In the context of construction, a realist orientation can be justified, individually for each scientist who maintains it, as the best means for extending and systematizing the results of scientific inquiry; the justification requires reference to the content of the scientific field and to the problems involved in extending it, but not to the ethos of the community of practitioners. This feature of the present account is proposed as a complement to Kuhn's, rather than as an alternative, because, again, the difference is primarily one of emphasis rather than one of substance. Both Kuhn's account and mine imply that the conduct of science will be characterized by commitment, occasional devaluation of the claims of logic, and selective blindness. But neither implies irrationalism, unless 'reason' is equated with 'logic'; and it has been apparent for some time that no such equation can be made.

20. A list of intellectual sources, however, as opposed to strictly thematic ones, would have to include the cogent analyses of Popper (esp. 1959, 1963) at its head. The account given here stems in large part from an attempt to work out the implications of Popper's well known dictum that "We learn from our mistakes." This account diverges from Popper's in substance, of course, principally because I am convinced (and have tried implicitly to show) that the attempt to equate 'mistakes' with (or subordinate them to) 'false statements' is erroneous. It is only people, and not statements, that make mistakes, and people cannot be separated from their mistakes in any way that is both simple and fruitful. The way that we learn from mistakes--either our own or somebody else's--is not by establishing a procedure for exposing them as quickly as possible but, on the contrary, by pressing

them on as far as possible until their mistaken character becomes self-evident. In such a way the value of the mistaken procedure, as well as that which makes it mistaken, can be determined. Such a course of action cannot, however, be codified as a set of explicit rules or criteria, because it requires and depends upon the selective blindness and concomitant selective intensity of focus entailed by commitment to the truth of a preferred position.

21. As pointed out in Chapter 2, the various neobehaviourist theories each had preferred areas of application, sets of problems which they were particularly well designed to handle. It was in connection with the claims of each to generality that competition and experimental criticism were concentrated.

22. According to the distinction between intervening variables and hypothetical constructs proposed by MacCorquodale and Meehl (1948), the former comprise mere labels for an observed functional relationship. They are entirely specified by the independent and dependent variables in terms of which they are defined, and have no status apart from a summary descriptive one. Hypothetical constructs, by contrast, have 'excess meaning' over and above the operations involved in their specification (e.g., as members of a postulate set), and thus refer to entities or processes presumed to function independently, outside the theory. The contrast is not so great as it seems, however, for hypothetical constructs are supposed to be permitted to function within a theory only insofar as their characteristics are specified in the postulates of the theory. Like other theoretical illata, therefore, hypothetical constructs and intervening variables alike are required to have their theory-relevant meaning specified, albeit in different ways, entirely within the theory.

23. It was on this basis that Watson declared Wundt's introspective psychology to be infused with "the religious mind-body problem" (see the quotation on p.61).

24. The pragmatist writings of James and Dewey were not closely addressed at first to scientific concerns; they could be appreciated as kin to explicitly positivist analyses of science only by those already engaged in the latter (as recounted by Frank, 1949a). Pierce's writings were very close both in spirit and in detail to later logical positivist analyses (Ayer, 1968), but were almost completely unknown before their publication as collected papers in the 1930s.

25. Again, cf. Watson's statement that behaviourism "may never make a pretense of being a system. Indeed systems in every scientific field are out of date (1924; 1961 ed., p. 18)." The complete quotation appears on p. 67.

Footnotes to Chapter 6.

1. It might be worth pointing out that Skinner's version of operationism, as depicted in his contribution to the Psychological Review Symposium on Operationism (Skinner, 1945), is different from both Bridgman's and Stevens'; the latter's is an extension of Tolman's definitional procedure for intervening variables and is most typical in neobehaviourism. As described previously, Bridgman's operationism requires a statement of the operations involved in measuring the quantity of something. Stevens' requires a statement of the observations stipulated as necessary for postulating or inferring its existence or presence. Skinner's operationism, by contrast, requires a description of the circumstances under which the name of the thing is emitted, or, more generally, of the situation in which a concept is employed. Skinner's operationism is thus, like his theories, more explicitly descriptive

than that typical of the other varieties of neobehaviourism.

2. The two sets of discriminated responses are not completely comparable in this experiment, since for the first set reinforcement is available on every trial--the discrimination is between two S+'s-- while for the second set reinforcement is available only on those correctly discriminated trials in which the dog runs down the runway-- the discrimination is between an S+ and an S-. This consideration does not materially affect the argument however; the factorial design is crucial here as permitting the systematic varying of the parameters.

3. The fact that the discriminated tonal sources were vertically rather than horizontally separated precludes any easy recourse to orienting responses as accounting for the differential ease of discrimination.

4. It might be objected that such experiments as these do not demonstrate the invalidity of the assumption of environmental generality, but rather show that the modifications in descriptive terminology necessary for application of the assumption have not been met. That is, such experiments indicate that the modifications necessary for extrapolating the descriptive schema to other specific controlled environments have not been sufficiently made, but do not indicate that if such modifications were to be made the resulting descriptive schema would fail to be applicable to all environments. This objection may be valid in principle, but it undercuts itself in that it is possible to simulate any given feature of the natural environment, apart from its complexity, in some kind of controlled operant environment or Skinner box. If the modifications necessary for extrapolating the descriptive schema had to be established and validated separately for every possible variety of Skinner box, the

requirement would amount to that of validating the descriptive schema separately for every separable feature of the environment, and hence would negate the assumption of generality itself.

5. The same judgment applies to the systematizations of the other neobehaviourists of course, insofar as they claimed that they were discovering perfectly general laws of behaviour. With regard to application of learning theory principles, however, the judgment does not apply quite so sharply to the others as to Skinner, inasmuch as they did not extend their experimental analyses to cover the fine grain of human behaviour in nearly so extreme a manner as Skinner has done. The schematic analyses of human behaviour advanced by the 'neo-neobehaviourists' (e.g., Dollard & Miller, 1950) are more self-consciously metaphorical and tentative than Skinner's.

6. However, it is true that Skinner was, of all the neobehaviourists, the most consistent and assertive in declaring the positivist character of his psychology (in Skinner, 1938).

7. This statement is obviously an elliptical rephrasing of Hume's phenomenalistic definition of causality as a 'constant conjunction of events'. The point of this way of putting it, as will be seen, relates to how we go about specifying an 'event' as having occurred on two separate occasions.

8. It may be worth noticing also in this regard that Skinner's serious interest in developing and refining the techniques of behaviour therapy--a term, incidentally, which Skinner seems to have coined--antedates the interest of many other behaviourist psychologists (Skinner & Lindsley, 1954), and was expressed well before Wolpe's first (1958) systematic writings appeared, although after Wolpe began practicing the art and reporting case studies (1952).

9. Very much lesser in some cases, it is true, because few experimental psychologists, apart from some of those influenced by Skinner, are systematically directed toward considering the detailed observation of the behaviour of their experimental subjects as the most important part of their professional activity.

10. The only informative discussion which I have seen on the subject of the interpenetration--rather than the possibility of coexistence--of phenomenology and behaviourism is that of Day (1970). Day does not go so far as to suggest, however, that the incorporation of a phenomenological viewpoint into behaviourism could take place or has taken place at as basic a level as I have represented it.

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(Note: Dates in parentheses after an author's name refer to the original date of publication, and are included when relevant to the argument in the text. Complete references are to the editions used.)

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Appendix

'Behaviourism and Positivism', by B. D. Mackenzie.

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BEHAVIOURISM AND POSITIVISM

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The past few years have seen a mounting disillusionment with and rejection of behaviourism as a basis for psychology. This mounting disillusionment manifests itself in many different forms: the publication of increasingly trenchant conceptual and methodological critiques of behaviourism (11, 23, 31, 42), the intensification of philosophical debate on behaviourism (e.g., 46), the attrition in the ranks of both well-known and unknown behaviourists (e.g., Sigmund Koch, George Miller, O. H. Mowrer, D. S. Palermo), the strong and vigorous development of the so-called "third force in Psychology" of Rogers, Maslow, and others (26, 33, 34), and perhaps most importantly, a gradual change in the kind of articles published in the hitherto mainstream behaviouristic journals.¹

Behaviouristically oriented psychologists have, in some cases, been quick to sense and respond to the attack. Such works as Kantor's *Scientific evolution of psychology* (16), Esper's *A history of psychology* (9), Skinner's *Contingencies of reinforcement: A theoretical analysis* (39), and Smith's *Behavior and conscious experience* (40) all attempt to show that behaviourism, or something very like behaviourism, is the only possible scientific orientation for psychology, and that it can quite robustly serve as a guide to any and all psychological phenomena. But these works, too, are evidence in themselves of the decline of behaviourism. They all are defensive works, and they all promote brands of "behaviourism" which are nearly unrecognizable. Skinner's radical behaviourist credo (39), reprinted in part from an earlier work (38), was originally described as "so extraordinarily libertarian . . . that one begins to wonder what the actual defining characteristics of the behaviourist thesis or the behaviourist method might be in his particular case (S. Koch, quoted in 46, p. 98)." The other three books promote various forms of closer relationship between psychology and biology, in the avowed hope that such closer connections will cleanse behaviourism (and psychology) of the last traces of psychic fictions. It is taken as almost an article of faith that a new and indomitable behaviourism will emerge from this process.

The question arises: if behaviourism is surrendering its hegemony, however unwillingly, how has it come to be forced to do so? Palermo (30) suggests that the downfall of behaviourism began in earnest with a dissertation by Kuenne (20), in which she showed that, for any behaviourist theory, there were unaccountable differences in verbal transposition behaviour between younger and older children. Palermo's claim at first appears unfounded; accommodation to more anomalous phenomena than Kuenne's had cheerfully been made within the behaviourist framework before Kuenne published.

¹In a discussion of a symposium on imagery in children's learning recently published in the *Psychological Bulletin* (30), D. S. Palermo observes "Some 15 years ago, when I was a year from completing my graduate work, . . . proposing a symposium on imagery at a psychological convention might have been considered a joke. Most hard-nosed experimental psychologists probably would not even have set aside their copies of *Modern learning theory* . . . long enough to notice such a symposium."

Palermo's claim makes more sense, however, in the light of the general historical analysis he provides. His article is of interest as one of the first explicitly "post-Kuhnian" analyses in psychology. T. S. Kuhn's theory of scientific progress (21) is by now well known; "normal" or everyday scientific activity in a given field is guided by a paradigm, or outstanding scientific achievement, which implicitly defines the methodology, conceptual structure, and problem areas of scientific research. This paradigm-based research is devoted to solving specific problems related to, and following from, the paradigm. Eventually one or more research problems of central importance to the paradigm proves intractable to paradigm-based research. These "anomalies" provoke a "crisis-state" in research; unsatisfactory attempts are made to resolve the anomalies by means of *ad hoc* additions to the paradigm-based theory, the paradigm comes to be severely questioned, and research proceeds on a relatively undirected basis for a time. Finally the anomaly is solved, at least in part by a non-paradigm-based piece of research; this achievement, or one following from it, forms the basis for the next paradigm.

Palermo has accepted Kuhn's analysis and applied it to psychology. Behaviourism is a paradigm-based research programme with classical conditioning (or, later, S-R conceptualizations) as its paradigmatic heart. Research was stimulated and guided by this paradigm into the familiar channels of S-R learning theories, drive-reduction theories of motivation, mechanical "mediating response" theories of memory and cognition, etc. The frontiers of behaviourist experimentation and theorizing were in the fields of complex human functioning, and ambitious attempts to extend behaviouristic theories to these fields were made by such pioneers as Miller and Dollard (8, 27), Rotter (36), Bandura and Walters (1), and Staats and Staats (41). Predictably, it was in these frontier areas that major experimental anomalies began to appear. The first was that of Kuenne (20), but Palermo goes on to specify others coming from the work of Harlow on curiosity and love (13, 14), Rock and Estes on one trial learning (10, 32), Olds on brain stimulation (29), etc. The work of Chomsky (e.g., 7) marks, according to Palermo, the first signs of the emergence of a new paradigm, one characterized by a "mentalistic and rationalistic orientation (30, p. 416)."

Palermo's analysis is simple, rational, and consistent with a growing trend to view the succession of scientific hegemonies as determined more by factors relating to the sociology of science than by those relating to scientific activity itself. The selection of Kuenne's dissertation as the beginning of the end for behaviourism makes more sense when viewed from the standpoint of Palermo's extension of Kuhn's theory. Kuenne was working within the behaviourist framework, at a behaviourist laboratory, and should have found good behaviourist answers. Instead, "... older children did not transpose in the same manner as the younger children. The latter behaved in much the same way as rats, as expected from the theory current at the time, but there was something peculiar and theoretically difficult to handle about the older children (30, p. 416)." If behaviourism is, in Kuhn's sense, a paradigm, then Kuenne's findings must be, in Kuhn's sense, an anomaly. The identification of Kuenne's work as a serious anomaly, that is, as the starting point of the crisis, cannot be made until after the crisis is in full progress of course, but this limitation on identification is customary.

THE CONTENT OF PARADIGMS

Is behaviourism, or the foundation of behaviourism, most appropriately viewed as a paradigm? At first sight it would seem so. Thorndike's puzzle-box studies and Pavlov's classical conditioning experiments appear to fit Kuhn's description of a paradigm as "one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice (21, p. 10)." They seem, furthermore, to have two characteristics Kuhn considers essential in a paradigm. "Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve (21, p. 10)."

Still, there is a fundamental difference in scientific status between Thorndike's and Pavlov's researches, and the research achievements that Kuhn provides as examples of paradigms. The difference has to do with the relative importance of the substantive and the methodological components of the scientific achievements. The significance of the paradigm as a scientific achievement is that it solves a problem: it answers particular questions about the world and is, primarily, a major substantive contribution to its field. Matters of methodology, conceptual orientation, and the definition of important problem areas are determined implicitly by the paradigm. Whether the proponents of the new paradigm attempt to legislate methodology or not, it is the paradigm itself that both stimulates and justifies any methodological development.

The paradigm, then, is first and foremost, a major substantive scientific achievement. By a process of implication and even of direct modelling, the paradigm has a major effect on the entire structure of the field. But each effect it has is justified by reference to the substantive significance of the paradigm achievement, a significance which necessarily antedates the effects justified by it. Paradigms, that is, are defined by their content, not by their methodology. This point deserves some emphasis, because it does not describe the way behaviourism developed.

It is questionable whether there was a crisis in psychological research when Thorndike published "Animal intelligence" in 1898 (43). There certainly was one by the time Watson published "Psychology as the behaviorist views it" in 1913 (47), and his adoption of Pavlovian conditioning principles in 1916 (48) was specifically intended to resolve this crisis. But neither Thorndike's researches, nor Pavlov's as Watson used them, answered any major questions or solved any substantive problems in psychology. Instead, they proclaimed that human and animal functioning *could* be understood in a particular way, and promised that the use of a correct methodology *would* make psychology into a genuine science. Thorndike's and Pavlov's results were never of principal importance (indeed, in the case of Pavlov, they have always been difficult to replicate); it was their techniques, and the principles that followed from their techniques, that became central.

The revolution that produced behaviourism was, in short, a methodological revolution. Behaviourism was not born from a solution, even a tentative solution, to a major problem. It was born rather of an uncompromising faith in a particular objective methodology, a faith that (as is well known) required the rejection and denial of those phenomena and foci of research which could not be made compatible with the methodology.

Legislation thus played a rather larger role in the development of behaviouristic psychology than in the development of those sciences it tried to imitate. There was no outstanding achievement to refer back to as proof of the worth of the behaviourist approach, and as a source of methodological principles. The methodology itself was the starting point, and was justified only by appeal to the future and to other "objective" sciences.

The same factors that made legislation necessary, however, also made it ineffective. The hegemony of behaviourism has always been rather loose, as well as geographically isolated. Debate over fundamentals was never resolved. The lack of justification for the extreme methodological tenets of Watson forced behaviourism to become "neo-" very swiftly. Tolman's first exposition of "purposive behaviourism"—in effect, teleological behaviourism—appeared in 1922 (44), just six years after Watson began to promote Pavlovian conditioning principles. Tolman's *Purposive behavior in animals and men* (45), ten years later, went most of the way towards introducing mentalism. In the same year, Cannon's *The wisdom of the body* (6) climaxed his refutation of Watson's peripheralism. Environmentalism is often thought to have remained central to behaviouristic formulations somewhat longer. Even so, Hull in his *Principles of behavior* (15) was forced to list a dozen autonomous drives, and to imply that his list might well be incomplete. A list of primary drives that encompasses hunger, thirst, sex, maternal drive, and drives resulting from needs for air, to avoid pain, to maintain body temperature, to defecate, micturate, rest, sleep, and be active (15, pp. 59-60), treads perilously close to instinctivism.

Environmentalism, peripheralism, the rejection of teleological explanation and mentalism—all these supposed fundamentals of behaviourism were abandoned or greatly modified long before behaviourism passed its heyday. Rejection of them took place within the behaviourist tradition, but on a piecemeal and *ad hoc* basis. They were abandoned, not through a process of growth and development of the behaviourist paradigm, but through a reluctant and gradual response to the inadequacy of a methodology that had never had significant substantive justification.

In summary, behaviourism was not in anything like Kuhn's sense, a paradigm. It did not have a paradigmatic base, and it did not have the power to settle fundamental issues that is essential for the practice of paradigm-based normal science.

THE FUNCTION AND DYSFUNCTION OF POSITIVISM IN THE DEVELOPMENT OF SCIENTIFIC SYSTEMS

If behaviourism was not based on a paradigm, then what was it based on, and how did it come to its predominant status? It is widely recognized that behaviourism did not at first succeed entirely on its own merits. It was "one of those ideas that are blessed at birth by the zeitgeist (2, p. 32)." "Psychology was all ready for behaviourism . . . the times were ripe for more objectivity in psychology, and Watson was the agent of the times (4, p. 642)." The essence of behaviourism was, as implied above, its adoption of objective methods and orientations avowedly analogous to those of the physical sciences. And those orientations, at the time, emphasized mathematical and provisional nature of physical constructs; under the spur of the collapse of the Newtonian world-view they rejected any reference of constructs to a "metaphysical" underlying reality. The orientation of physics at the time was,

in a word, highly positivistic, and it was the adoption of this positivism, which was becoming an immensely popular and influential philosophical and scientific movement, that greatly helped ensure behaviourism's success.

It was this adoption of positivism that gave behaviourism, not just its emphasis on observables, but its rejection of anything purporting to lie behind the observables. At first the fact that it was positivism that psychology was adopting from physics was little recognized; following the introduction of Bridgman's operationism (5) the adoption of positivism became conscious and systematic. Operationism has always been taken more seriously in psychology than in physics. It was of great assistance in the development of neo-behaviourism, since it seemed to permit, while keeping staunchly within an objective and positivist framework, the introduction of any level of concept so long as the concept could be "operationally defined". Both the success and the characteristics of the behaviourist programme are thus largely attributable to the acceptance within psychology of a natural-science-based positivism. Positivism in psychology, however, is still something very different from positivism in physics, and this difference is of central importance to an understanding of behaviourism.

The development of positivism as an internal process within a scientific discipline is a cyclical historical phenomenon with a determinable social function (cf. 19). Its central characteristic is a systematic suspension of judgment concerning the reality of a particular explanatory system. Its function seems to be that of easing the transition from one explanatory system to another which replaces it. Positivism may arise when an old system, which is accepted and believed, comes to have its validity questioned; it makes possible the response to criticism that the system under attack is justified as a scientific system by its pragmatic success (by successfully predicting phenomena) rather than by its metaphysical success (by accurately reflecting reality). Positivism flowers when a new system is proposed to replace the old one; the resistance to accepting a new explanatory schema with potentially revolutionary metaphysical implications is at least as great as the reluctance to abandon the old one. Whether the proponents of the new system are positivistically oriented or not seems to make little difference. The claim comes to be made for the new system that it, too, is justified pragmatically,² that it is a mistake to see it as requiring a change in world-view. As the new system becomes accepted and commonplace, its positivism is tacitly and gradually abandoned; its principles for describing and accounting for phenomena come to be taken as descriptions of real processes. The positivistic approach to a system may never entirely die out, even after the system has become widely accepted, and is always considered a respectable, if somewhat overly cautious, scientific attitude. Its incidence in science, however, is very low during the period that the system is fully accepted. Positivism has philosophical roots, of course, in skeptical epistemology, and may continue to stimulate activity in philosophy throughout the "realistic" period of acceptance of a scientific system.

Examples of positivism are readily available in the history of science. Both the Eudoxian (heavenly spheres) and the Ptolemaic (equants and epicycles) theories

²From the standpoint of Kuhn's theory, such a claim must always be unjustified. The old theory, at the time of its death, is always able to do more than the new theory at the time of its birth.

of astronomy were proposed simply as mathematical devices to "save the phenomena"—to enable prediction of the observed motions of the planets without regard to their hypothetical actual behaviour. This early positivism was given rational support by the argument that it was impossible to make astronomy a natural science since its objects were totally inaccessible to close observation and experiment; therefore, any statements concerning the "real" nature of the stars and their motions could not even in principle be justified. Nevertheless, an amalgam of the Eudoxian and the Ptolemaic theories gradually came to be considered true, rather than merely efficient. Copernicus, in his turn, believed in his heliocentric hypothesis, but much of the early defense of his system emphasized a positivistic justification similar to the ancient one (35). Newton was never satisfied with the account he gave of gravitation, and cautioned his readers to consider his formulation as merely a mathematical description. They did not do so, and gravity slowly became accepted as a fundamental property of matter.

The emergence of positivism towards the end of the nineteenth century displays a similar pattern. A few physicists, such as Ostwald and Priestley, had resolutely kept to a positivist conception of Dalton's atomic theory. In general, however, although the scientific temper of the period was aggressively tough-minded, it was a tough-mindedness of materialism rather than of positivism. A thoroughgoing positivism started to be widely acceptable only with the publication of Mach's *Science of mechanics* (24) in 1883 (Eng. tr. 1893). The acceptance of Mach's positivism and of the doctrines that followed from it was based on the gradual buildup of anomalies, and the gradual loss of faith, in the Newtonian scientific system. The theories—quantum mechanics and relativity theory—that followed from all these anomalies have continued frequently to be presented as positivistically based theories (although Einstein's own distaste for positivism is well known). Koyré, however, confidently predicts that the "positivist phase of renouncement" will once again give way to a realism (19). There are indications that he is right, that physicists are increasingly coming to assign greater realistic significance to their postulates (22, 28).

Psychological positivism, then, was adopted from a science wherein it currently appears as a symptom and concomitant of revolutionary change. But the revolutionary change is always from one world-system that has become inadequate to another developed to redress the inadequacies. In behaviouristic psychology this could have happened but did not. The Wundtian, elementaristic, associationistic, paradigm was proving inadequate and a Darwinian functionalist paradigm for a time seemed to be the replacement. However, the Darwinian emphasis on unconscious wellsprings of behaviour and on instinctual mechanisms was unacceptable to the dogmatic objectivism of developing behaviourism. Positivism lost, in its transition from physics, its function of masking and rationalizing underlying entities, and served instead to abolish them. The Darwinian paradigm was instead exploited by Freud and, much later, by the ethologists. The Darwinian approach did not disappear altogether in America of course, but was used only by those, such as McDougall and Yerkes, who eventually came to reject the behaviourist orthodoxy. Darwinism in American psychology thus never achieved the status of a paradigm and never realized its potential.

The mainstream of American psychology thus placed itself in the curious

position of adopting a methodology appropriate to paradigm shifts, while the manner of adoption entailed having no paradigm to shift to. This is the failure of behaviourism, that it had no world-view to grow up into, to guide research, to provide substance to its orthodoxy; it restricted itself instead to a methodology which is productive only when there is a world-view beneath it, waiting to emerge.

This is not to say that behaviourism was entirely without underlying, guiding, metaphysical principles. It had them, and they were the same elementaristic, associationistic ones derived from British empiricism as were present in the rejected Wundtian paradigm. However, the possible implications of these principles for a new model of the nature of psychological processes could not be realized, because they had become so widespread, vague, and generally disseminated throughout the scientific culture that their substance had been reduced to the general methodological maxim of "analyze everything into its components." This reduction of the metaphysical principles to the level of methodological maxims was reinforced, of course, by the growing positivism of the scientific culture. In addition, the kind of philosophical positivism which was starting to develop at the time, and which eventually became the logical positivism of the early Vienna circle, was itself inclined strongly towards the same kind of empiricist elementarism (3). The principles did influence much behaviourist research, but they were not essential to behaviourism, and could be tacitly abandoned when necessary, as the elementarism was abandoned in Tolman's theory. They served to give the appearance of content to the positivist orthodoxy, but they expressed no new principles or insights, and were never sufficiently strong or stimulating to initiate an emergence from the positivism.

POSITIVISM AS SCIENTIFIC ORTHODOXY

Serious consequences arise when positivism is institutionalized as the assumed content of a discipline. The development of the discipline is restricted, since the positivist orthodoxy, like all orthodoxies, resists change that is anything but a development of itself. Since positivism is substantively empty, a positivist orthodoxy resists any genuine development. It has two important defenses against change.

First, the orthodoxy has an effective criticism available to counter any proposed world-view. By the tenets of positivism, which are taken to be the tenets of science, any world-view is meaningless (unverifiable), and hence unscientific. This criticism of any non-positivistic position, that it is unscientific, is simple to apply and is sufficient to invalidate any such position in the judgment of all those who accept the orthodoxy.

Second, since a positivist orthodoxy has no substantive core, it is not falsifiable; that is, any empirically verifiable statement is consistent with it. The orthodoxy is thus totally pluralistic, and any finding that seems anomalous to a theory consistent with the orthodoxy can in fact be accommodated within it.

The combination of these two defense mechanisms provides a devastating defense against any scientific system that challenges the orthodoxy: the empirical findings of the challenging system can be accommodated within the orthodoxy³,

³It is very difficult for any position to be entirely content-free. In practice, therefore, an additional technique is employed of ignoring empirical findings that cannot be assimilated.

and the theoretical formulations, to the extent that they are not compatible with a positivistic orientation, can be dismissed as unscientific.

These defensive reactions are familiar occurrences within behaviourism. They are the basis for much of the behaviourist criticism of psychoanalytic theory (e.g., 37); the attempt of Dollard and Miller (8) to translate Freudian theory into S-R terms was explicitly designed so as to accommodate the range of Freudian findings within a behaviourist framework while rejecting the unique components of Freudian theory as unscientific. Maltzman provides an explicit version of this defense, or counter-attack, in discussing behaviourist vs. cognitive (i.e., mentalistic) treatments of awareness in verbal conditioning.

No cognitive psychologist has made any discoveries, obtained any empirical laws, uncovered new experimental variables which logically could not be treated within some behaviourist theory. . . . The difference between the cognitive psychologist and the behaviourist is in the logical status of their respective theories. For the behaviourist awareness is a defined concept. For the cognitive psychologist awareness is a primitive or undefined term, despite disclaimers about operational definitions and converging operations. . . . Psychology can manage without such a *ding an sich* (25, p. 162-3).

In this double-barreled, almost sophistic technique of reply to criticism, behaviourism is rather similar to its philosophical cousin linguistic philosophy, which could be identified as another modern case of positivism institutionalized as the pseudo-content of a discipline (cf. 12). It is the imperviousness to substantive criticism of behaviourism that gives significance to the remark of Koch: "I suspect that there is a class of positions that are wrong but not refutable and that behaviourism may be in such a class (17, p. 6)." Koch relates his remark separately to the metaphysical and to the methodological aspects of behaviourism. The analysis presented above, however, implies that the methodological and metaphysical components cannot be considered separately, or even be properly identified apart from each other, for behaviourism has succeeded in making its methodology into a kind of metaphysic.

The resistance of behaviourism to change and criticism has not, of course, made change impossible or criticism ineffective. What it has done is to delay change, and for a time to force criticism and change to be piecemeal. It has strongly hindered the development of a new paradigm. Its techniques for doing so, from the defensive viewpoint, were described above. From the viewpoint of the initiators of change, it has also done so by providing little specifically to react against. Behaviourism's anomalies are not Kuhn's anomalies; *ad hoc* additions designed to counter the anomalies are very difficult to distinguish from apparently genuine extensions of theory. Behaviourism has thus not only failed to provide an adequate systematization within psychology. It has even failed to present central problems for the attention of a possible successor.

The decline of behaviourism is not being ushered in by the growth of anomalies, by the appearance of a new paradigm that gives the anomalies a more central position.⁴ Instead, a growing awareness of the emptiness of the behaviourist pro-

⁴As was the case, for instance, in the development of gestalt psychology out of structuralism.

gramme is giving rise within the behaviourist ranks to a feeling of weariness and disillusionment. At the same time, the tentative programmes instituted by those working independently of the behaviourist orthodoxy are beginning to bear fruit and to attract more and more workers. Common to behaviourists and others is the feeling that behaviourism has simply failed to make good its promise.

It seems necessary, therefore, for American psychology to revert to an explicitly pre-paradigm position for a time. Behaviourism, during the period of its domination, prevented the development of a paradigm, and none of the recent competing schools is yet in a position to advance one. The conclusion of Koch (18), that the unity and scientific coherence produced by adherence to a paradigm are impossible within psychology, is obviously premature. His conclusion is based on the assumption that behaviourism, despite its fundamental inadequacies, is a scientific system, and that the failure of behaviourism to become systematically viable must therefore preclude scientific coherence and unification on the part of any successor. In contrast, the analysis presented here concludes that behaviourism is not a scientific system and has hitherto prevented one from developing.

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